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THE Council have to regret the delay which has occurred in the publication of the present Part. Various circumstances, over which they had no control, have combined to occasion this. The succeeding Part is in a very advanced stage and will appear in a few weeks: it will contain:—

THE GRAVESEND PIER	PLATES.	BY W. TIERNEY CLARK.
ON LOCOMOTIVE ENGINES		BY PROFESSOR BARLOW.
THE EXPANSION OF IRON AND STONE ARCHES		BY G. RENNIE.
A MACHINE FOR CUTTING RAILWAY BARS	PLATE .	BY J. GLYNN.
ON THE MONTROSE SUSPENSION BRIDGE	PLATE .	BY COLONEL PASLEY.
ON THE SINKING OF THE WELL AT THE RESERVOIR OF THE NEW RIVER COMPANY IN THE HAMPSTEAD ROAD .	PLATE .	BY R. W. MYLNE.
ON THE LOCOMOTIVE ENGINES OF THE LONDON AND BIRMINGHAM RAILWAY	PLATE .	BY E. BURY.

ERRATA.

Page 58, line 4, after the words "non-condensing engine," add "working at the pressure of Experiment III."

—— 66, line 17, after the words "by this class of engine," add "at the two pressures cited."

—— 137, line 19, for "in the two experiments with the Lion," read "in Experiments I. and II. with the Neptune."

II.—*On Steam-Boilers and Steam-Engines.*

By JOSIAH PARKES, M.Inst.C.E.

PART II.

ON STEAM-ENGINES, PRINCIPALLY WITH REFERENCE TO THEIR CONSUMPTION OF STEAM, AND FUEL.

In the introduction to this branch of my subject, (page 2,) I have already expressed an opinion, that the weight of water, in the shape of steam, which passes through the cylinder of an engine, and produces a given effect, is the best practical measure of the dynamic efficiency of that steam; and that it is the most certain criterion of the economy of different classes of engine in their use of heat.

The habit of comparing the horse-power, duty, tractive effect, or work of an engine—however denominated—with the weight of fuel consumed in doing that work, confounds the results of two dissimilar and unconnected operations; it has mystified the subject; it has greatly tended to obscure our knowledge of the actual degree of excellence attained by the steam-engine; and has retarded the extension of the most effective system of applying steam as a motive force. The generation, and application of steam are distinct problems; they require to be separately treated, and their results to be separately stated. It is the economy of steam which constitutes the dynamic perfection of an engine; it is the economy of heat in supplying steam to an engine, which constitutes the evaporative perfection of a boiler; and it is only by distinguishing the effects of each, that the value of any change of practice, in either department, can be correctly ascertained. These economic properties are totally independent of each other, they may co-exist in the same engine in a maximum amount, or be possessed by it in very different degrees. An acquaintance with the weight of fuel burnt, with the weight of water evaporated, and with the mechanical effect realized, enables us to determine, accurately, the degree of perfection in which these properties are respectively found, in particular classes of engine,

and in particular engines of every class. These are important practical problems, to the solution of which, the attention of engineers and employers of engines should be especially directed. Their solution does not involve the application of extraordinary scientific acquirements, nor does it require experiments of a very nice or difficult nature, nor the use of corrections or data drawn from theoretical sources; any person endowed with common powers of observation and experimental tact, is as capable of discovering the position of an engine, in the scale of economy, as if he were gifted with the genius of a Newton.

So long as engines were constructed chiefly by one maker, their varieties few, and their forms nearly identical, an evaluation of their particular economy, once made, was a nearly sufficient indication of the performance of all; but new forms of engine, new, or extended principles of applying steam have gradually established themselves; and a correct expression, or measure of the performance resulting from these several systems, has become, both for practical purposes and scientific advancement, a matter of deep and acknowledged interest.

It is, therefore, singular that a research into the exact state of the practice, as regards the production and consumption of power, by the different varieties of the steam-engine, should have been so little attended to by engineers.

It has been my humble endeavour to contribute towards the fulfilment of this object, by ascertaining the requisite facts on engines within my reach, and by reducing such other authentic experiments as I could meet with, to the same common standards of comparison. Sufficiently accurate experiments (though not so numerous as might be wished) enable me to investigate several classes of engine, and to determine their relative expenditure of power and heat. They comprise the atmospheric, the stationary non-condensing or high-pressure, the low-pressure condensing, and the Cornish high-pressure, expansive, pumping engines. Table VI. contains the facts established on these four varieties.

Locomotive engines, and their performance, have also been submitted to frequent analysis, but, for reasons which will fully appear in the sequel, I have deemed it requisite to classify them in separate tables; it being first necessary to ascertain the degree of accuracy, to which the various experiments on this class of engine are entitled, before admitting the results obtained from them to a place beside those which deserve implicit faith. It will be seen, too, that the determination of the expenditure of power, for given effects, by other engines, was a necessary step, and will have largely contributed towards the successful examination of the power and performance of the locomotive.

OF THE METHODS EMPLOYED TO DETERMINE THE POWER OF ENGINES.

There are two methods, in common use, for determining the effective power of steam-engines; the one, by ascertaining the resistance overcome; the other, by employing some instrument, such as the *indicator*, to exhibit the load upon the piston; the velocity of the latter being noted at the same time.

The first method is strictly applicable to pumping engines only, or to engines raising appreciable weights; the second presents, perhaps, as near an approximation to the truth, when applied to rotative engines, as that class of engine is susceptible of. It informs us of the amount of force requisite to overcome the friction of the engine, isolated, and of the total force employed under its load. It is customary to designate the difference of these two amounts, the *effective power* of the engine. This definition is not, however, strictly correct, as the additional friction, to which the moving parts of an engine are subjected by the load, is thus involved in the expression, and reckoned as a part of useful effect; by that amount, therefore, inconsiderable though it may be, the determination of effective power, by this method, is in excess.

On the other hand, the power, deduced from the duty of a pumping engine, falls short of the real effective power of the steam used. This engine, when applied in the best manner, does not act directly on the water; the force of the steam is employed to lift the pump rods, the weight of which, together with their friction, that of the accessory balance beams, or *bobs*, &c., and of the water in the pipes, constitute the true load upon the engine; the duty, therefore, ascertained by the column of water displaced, does not represent the entire effective power of the steam. The value of these surplus frictions above the proper friction of the engine, unloaded, should be added to the duty, for the purpose of bringing the Cornish, or other pumping engine, to an exact comparison with a rotative one, the effective power of which has been ascertained by the indicator.

It is thus evident that *useful effect* is not always synonymous with, nor a true measure of *effective power*. The duty is the true useful effect of a Cornish engine. The method of ascertaining it is the fairest possible, for practical purposes, and it will be apparent that the errors in estimating effective power are against the Cornish, and in favour of the rotative engines. I have thought it advisable to point out the mutual bearing of these errors in computing effective power, in order to remove all suspicion, that the Cornish engine may

owe any portion of its greatly superior performance to an exaggerated estimate of the duty from which its effective power is deduced.

The indicator may be usefully applied to a Cornish engine, to ascertain its *absolute* power; but as that engine cannot be isolated from its load, so as to determine the separate value of its own friction, the indications of the instrument would not serve to place the estimation of its effective power, on an equality with that obtained by them from a rotative engine. The indicator, however, by ascertaining the total pressure on the pistons of both engines, would inform us of the absolute and relative power derived from the expansive, and unexpansive systems of using steam, by enabling us to compare the loads deduced from the mean pressure on the pistons, with the weights of water as steam, which have respectively passed through the cylinders, and overcome those loads. But, in order to obtain correct results, it is necessary to apply the instrument to the vacuum, as well as to the steam side of the piston of a single engine, it being well known that the amount of vacuum in the cylinder is not identical with that in the condenser, nor instantaneously produced; it being also well known that the rapidity with which the vacuum is formed, affects the power of an engine in no inconsiderable degree. It was the perception of the influence of these phenomena on power, and economy, which led Watt to invent and apply the indicator, as an accurate means of ascertaining the variable and incrementary amounts, both of pressure and exhaustion, which take place *within* the cylinder, during a stroke of the piston, rather than confide in deductions from uncertain data acquired from *without* the cylinder.

There is a third method, by which the absolute power of steam, communicated to an engine, can be deduced; but, as its value depends on a perfect accordance between the results of experimental and practical science,—an accordance yet unascertained,—and since many precautions are requisite to secure true results from this test, it has been seldom resorted to by practical men.

The volume of water converted into steam, being known, and also the volume described by the motion of the piston within the cylinder, the load upon the piston is deducible from a knowledge of the elastic force of steam, corresponding with the ratio which the volumes of water and steam, consumed by the engine, bear to each other. It being assumed that the densities, and consequent pressures of steam are correctly assigned to these relative volumes, it is necessary, for practical use, to detect and fix the amounts of those portions of the steam, which are ineffective as power. These quantities vary in different

engines, and must be ascertained for each engine. A portion of the steam is condensed by contact with surfaces of lower temperature than itself; another portion is useless, from its filling the spaces contained between the piston, the cover and bottom of the cylinder, and steam-valves. The proportion of these ineffective to the effective quantities must, consequently, be deducted from the volume of water, before its true ratio to the volume of effective steam can be found; and care must be taken that no loss of water, or steam, take place, in other ways, during the experiment.

There is another source of inaccuracy, which, if undetected, would altogether vitiate this test; it frequently exists to a considerable extent, unsuspected; or, if suspected, its amount eludes determination. I refer to *priming*. The power of an engine, whose boilers are subject to prime, cannot be investigated with success by the method of the volume of steam, as will hereafter appear in the examination of the locomotive engine. No value can be attached to this method, unless the steam be chemically pure; unless it be free from particles of unvaporized water, mechanically suspended in it, and accompanying it into the cylinder. But in all cases where the steam is pure, this test may be advantageously used (under the foregoing precautions) to compare the absolute power of an engine, as exhibited by the indicator, or by known resistances, with the absolute power resulting from the whole force of the steam which has operated upon the piston *.

OF THE MEASURES OF EFFECT.

The performance of pumping engines is commonly defined by the weight in pounds which has been raised one foot; and this quantity, being divided by the number of fixed measures, or weights of fuel, which have been expended in raising it, gives a product termed the *duty;* a phrase which very significantly expresses its meaning. The relative performance of different engines is thus conveniently found by a comparison of the duty obtained from equal measures, or weights of fuel.

The performance of rotative engines is commonly referred to the weight in pounds which has been raised one foot, during a minute of time; which sum,

* It is very desirable that a series of experiments should be instituted to demonstrate, if possible, the degree of accordance which may subsist between results obtained by this process on the large scale of practice, from the same engine working under greatly different pressures, and the determinations of chemical philosophers.

divided by 33,000 pounds, gives the expression of effect in terms of *horses-power;* a conventional phrase, which, however inconsistent it may be in its application to many of the uses of the steam-engine, and however vague it may be as a measure of the power of a horse, is, nevertheless, as good as any other expression we could adopt, for the purpose of bringing the economy of various engines to a common standard of comparison.

It is usual to compare the relative performance of engines, of the rotative class, by the number of pounds of fuel consumed by each horse-power in an hour. The weight of fuel thus ascertained, has, therefore, raised 1,980,000 pounds one foot in 60 minutes, and it is the duty done by that weight of fuel; whence, by simple proportion, the duty is obtained in terms of any other weight of fuel.

In like manner, by noting the time in which the duty of the pumping engine has been accomplished, it is readily reduced to terms of horses-power; so that the effects derived from engines, of both classes, are comparable under both denominations.

I have, accordingly, exhibited (Table VI., columns 8, 9, 10) the amount of duty of each engine as performed by 1 lb., 94 lbs. (or the Cornish bushel), and 112 lbs. of coal respectively, for reasons which will appear in the sequel. The weight of coal consumed per horse-power per hour is also shewn in column 15.

We have thus two expressions of the effect obtained by equal weights of coal from the engines under comparison; but those expressions convey no information as to the real expenditure of power in the production of a given effect.

OF THE EXPENDITURE OF POWER.

The ponderable element of steam is water; its consumption by an engine is appreciable; and it is now assumed, almost universally, that the sum of its imponderable element, heat, is a constant quantity, in steam of all specific gravities. The elastic force of steam is also generally assumed to be proportional to its density; thus, equal amounts of heat and water are expended in the generation of equal power, at whatever pressure steam be used by an engine. The correctness of these assumptions, founded on the experiments of many distinguished philosophers, will receive additional confirmation from some experiments of my own, purely practical, to be related hereafter *. Admitting, then,

* Vide page 70.

these properties of steam to be inseparable from its composition, the expenditure of both power and heat, is truly measured in all engines, by the weight of water consumed, as steam; and the effects produced, being known, and reduced to a common denomination, the weights of water so consumed indicate the positive, and relative efficiency of the steam in different engines.

Admitting, however, the supposition that the elementary constituents of steam may not yet be absolutely defined, with equal accuracy, throughout the whole range of its scale; and that the proportions of heat to water, at different pressures, may undergo changes not yet correctly ascertained, the weight of water as steam, expended in the production of a given effect, remains unimpaired in its value as a test of dynamic efficiency. It becomes, indeed, for these very reasons, the more valuable for practical uses, than a test derived from the consumption of any given measure of steam.

By knowing the evaporation from the boilers, and, consequently, the weight of water as steam which passes through an engine, we grasp the principal fact of practical consequence to the engineer; a fact which is free from all uncertainty in its nature; which involves no dispute as to its qualities or characteristics; which is, in no respect, mixed up with the chemistry of steam; which is independent of all theory, and requires no correction to determine its value. So long, too, as the steam is taken immediately from above the surface of the water in the boiler; and so long as it enters the cylinder of an engine unchanged, it is in that state which every one understands by the term steam, in its common acceptation; and the weight of water, which has passed from the boiler in that state, and produced a certain effect, appeals conclusively to the understanding as indicative, in a comparison of engines, of their respective economy in the expenditure of power.

I have, accordingly, computed the weight of water as steam equivalent to the production of a horse-power in each engine, and, also, the duty effected by one pound of steam. These sums (columns 11 and 16) denote the positive and relative *efficiency of the steam* in the different engines; and, in like manner, the positive and relative *efficiency of the fuel* is exhibited (columns 10 and 15).

Under the head *Comparative Economical Results* (columns 21 and 22), I have reduced these respective indices of efficiency to terms of the ratios which each of them bears to the results obtained on Watt's engine at the Albion Mills, which latter I have assumed as unity. The table therefore exhibits,

		Columns.
1.	The precise value of each engine in its use of steam . .	11 and 16
2.	The precise value of each engine in its use of coal . . .	10 and 15
3.	The precise value of the boiling apparatus attached to each engine, as a generator of steam	17
4.	The comparative efficiency of the steam, or economy of power, in each engine	21
5.	The comparative efficiency of the fuel, or economy of the combustible, in each engine	22

OF THE PROPORTION OF BOILERS TO ENGINES.

Having thus arrived at an exact knowledge of the weight of water as steam, consumed by each engine in the production of equal effects; or, in other words, of the power resulting from the expenditure of equal weights of water as steam, I am able to connect the boilers with the engines, and to shew the relative extents of heated surface which have been employed to furnish their power; and it will presently be seen how insufficient equal measures of surface would be, to supply equal power, with equal economy, to different classes of engine.

The effective horse-power of each engine, at the time of the experiment, is shewn in column 12; and the heated surface of boiler, per horse-power, in column 18, obtained from the total area belonging to the same engines, given in the table of boilers (column 12, page 45). It would appear, by this comparison, that the Cornish engineers employ about three times more surface than Watt did, reckoned on the horse-power of engine; but, column 21 informs us that the horse-power is produced in a Cornish engine, (taking the mean of experiments VI. and VII.,) by somewhat less than two-fifths of the steam consumed at the Albion Mills: thus, the Cornish engine, for equal nominal power, has nearly eight times as large a boiler surface as that allowed by Watt's practice. But this measure exhibits no true comparison of the proportion of generative area to the power expended by different engines; for, the functions of the former are irrespective of the engine, and have reference only to the fuel burnt, and to the water evaporated. By column 15 it is seen that the Cornish engine (using the mean before stated) consumed only $2\frac{1}{2}$ pounds, whilst Watt's engine

consumed $8\frac{1}{2}$ pounds of coal, per horse-power per hour. Dividing the heated areas of the boilers by these respective amounts, a product is obtained which shews the positive and relative extents of surface for each pound of coal burnt to supply the two engines, with equal power, in equal times. These results are set down in column 19, and the true relation of the boilers is found to be as 19 to 1, instead of 3 to 1.

Column 20 contains the heated area of the boilers, proportionate to the weights of water as steam, which have produced equal power, in equal times, in the two engines, which gives a ratio of nearly 15 to 1 to the Cornish, in excess over Watt's practice, instead of 8 to 1.

It will now be apparent why I could not assign the relation which subsists between the boiling apparatus of any particular engine as regards surface, and the power of that engine, until its consumption of steam and fuel were ascertained. It will also be seen that, if the Cornish engineers had followed a rule of boiler-surface, based on the mere nominal horse-power of engine, instead of wisely continuing to augment the proportion of boiler, whilst they were gradually reducing the expenditure of steam as power, they would have lost 3-10ths of the superior economy of their system.

These last measures, contained in columns 19 and 20, are, however, no fixed rules as regards the Cornish practice; they apply only to the particular instance cited; for, the proportionate boiler-surface necessarily varies with the degree of economy of steam and fuel. Experiments VIII. and IX., on two other Cornish engines, exhibit a large diminution in the consumption of fuel, and water as steam, for equal power, compared with experiments VI. and VII.; and, though I have not the particulars of the boilers, we may be well assured that their evaporation was fully equal to that from the others *; and, consequently, that their heated areas were in the same proportion. Assuming this to be the case, the surfaces of the latter, reduced to the terms of columns 19 and 20, would be larger; and, for experiment X., we should find them increased from the ratios of 19 and 15, as in experiments VI. and VII., to 27 and 21 to 1, in excess over Watt's. The performance of the expansive pumping engine is subject to fluctuations; and the measure of boilers, taken as above, necessarily changes with the power consumed; the one being a constant, and the other a variable quantity.

Well made rotative unexpansive engines, whether of the condensing or non-condensing kind, can vary but little in their respective consumption of steam,

* Vide page 63.

for a given power, at different periods; but their boilers, to produce steam with equal economy, must necessarily be proportioned to the weights of water, which have to be vaporized, in order to create the power. Thus, a high-pressure non-condensing engine would require $\frac{5}{7}$ths more generative area than the condensing engine, to be upon a par with it in calorific economy, as the former consumes $\frac{5}{7}$ths more steam than the latter, for equal effects.

All engines may be furnished with boilers possessing equal evaporative economy, that being a result derived from the amount of surface, and other qualities previously explained; and the best rule, if it be necessary to prescribe one so obvious, is to adopt that measure of surface which has afforded the greatest evaporative effect from a given weight of fuel. The weight of water as steam, consumed per horse-power per hour, being shewn in column 16, the total vaporization required in an hour, by any good engine of the classes examined, is obviously obtained by multiplying that weight by the number of horses-power of the engine in question; and the total generative area requisite for realizing the maximum effect hitherto obtained from coal, will be to employ as many superficial feet of heated surface, as pounds of water used per hour. This is the Cornish measure, as will appear by consulting the table of boilers, and is referable, strictly speaking, to the Cornish form, strength, and setting of boilers, rate of combustion of fuel, &c.

OBSERVATIONS ON THE EXPERIMENTS, AND THEIR RESULTS.

The first experiment in the table is by Smeaton, on his improved Newcomen engine.

This experiment is particularly interesting, as a record of the consumption of power, by a good specimen of the earliest steam-engine, which can be considered as having exerted any important influence on the economy of the arts. It was also the great predecessor and type of Watt's engine; whose improvements upon it consisted chiefly of additions affecting its economical use of steam, retaining the great principle of condensation, together with the mechanical structure, viz. cylinder, piston, beam, and self-acting valves; apparatus which continue in use at the present day, with the advantage only of superior workmanship, and of more perfect mechanical contrivances.

Newcomen's engine must ever be regarded by engineers with a species of veneration, as the great forefather, and founder of the whole race of steam-engines. The numerous, beautiful, and highly scientific combinations to which

the steam-engine owes its modern perfection, have always appeared to me to be entitled but to secondary claims in the award of merit, compared with that due to Newcomen and his compeers, who, in reality, created it. Prolific as the genius of Watt must be regarded; immeasurably important to mankind as were the consequences of his discoveries; we must not overlook the fact that the steam-engine was already invented; that it was already a mighty instrument of mechanical force; and, though Watt greatly reduced its expenditure of power by one contrivance, and, by another, made it applicable to rotative purposes—thereby enlarging the bounds of its usefulness beyond calculation—we must bear in mind that the great problem was already solved; and that, strictly speaking, Watt's improvements were only corollaries, requiring, doubtless, more than ordinary talent to apply them, yet they were improvements, not creations.

Following, in these remarks, the order of invention, rather than the order of the experiments in the table, it appears that the economy of Watt's rotative condensing engine, at the Albion Mills, (Experiment V.,) with reference to its consumption both of steam and fuel, was double that of Smeaton's Newcomen. The performance of that rotative engine, measured by the duty, corresponds, also, so nearly with the best work done by Watt's pumping engines in Cornwall*, that the comparison holds good for both kinds.

No improvement in the science of the unexpansive condensing engine has taken place since Watt's day; and there are, probably, very few engines of this class, which perform their work with so small a waste of steam, or fuel, as the one experimented upon at the Albion Mills, which approached very nearly to perfection in the use of power, obtainable from that principle.

The next great advance, in the economy of power and fuel, had its origin in Cornwall; the merit of it is wholly due to the Cornish engineers; to whom belongs, also, the merit, not only of having carried the system adopted by them for pumping engines, to a degree of perfection which eclipses the performance of every other description of engine, but of applying it to rotative purposes, with effects far exceeding those of the common condensing engine. The rise and progress of this vast improvement is so well traced by Messrs. Lean, in the useful and highly instructive compendium already referred to, that all observations from me would be a work of supererogation.

Experiments VI. and VII. exhibit, I believe for the first time, the precise

* Lean's Historical Statement of the Duty of Steam Engines in Cornwall, page 8; compiled for the British Association for the Advancement of Science, 1839.

amount of the superiority of this engine over others, and it is seen that, when doing an 80 million duty, it excels Watt's by $2\frac{1}{2}$, and Newcomen's engine 5 times, in economy of power; and, that it excels them $3\frac{1}{2}$ and 7 times respectively, in economy of fuel. It is also seen that a Cornish engine (Experiment IX.) has exceeded the same engines $4\frac{1}{2}$ and 9 times respectively, in economy of power; and $5\frac{3}{4}$ and $11\frac{3}{4}$ times respectively, in economy of fuel.

Such has been the progress of invention, estimated by economy in the expenditure of power, between the years 1772 and 1835.

It is not my intention, were it even in my power, to enter here into any analysis of the causes of the greatly superior effect derived from steam, when used on the Cornish system. My principal object has been to arrive at practical determinations of the consumption of power, considered apart from the consumption of fuel, by each engine. The doubts entertained by many persons of the accuracy of the Cornish statements of duty, and, consequently, of the effects derived from the use of expansive steam, are chiefly attributable to the Cornish method of rating the duty by the fuel, rather than by the power really expended. By separating these results, I hope that I shall have removed one great cause of distrust, which has certainly operated to retard the extension of the expansive system of using steam to several other classes of engine, to which it is well suited; and that I shall have cleared away obstacles to our arriving, at some not distant period, at a successful discrimination of the inherent value of the expansive action of steam, and of its value when combined with the mechanical action, and peculiar work of a steam-engine. These are problems, of a nature distinct from the practical facts now sought, and require very nice experiments for their solution. It has been customary, on the one hand, to assign the superiority of the Cornish engines solely to the use of steam of high elastic force greatly expanded; and, on the other hand, to dispute the value of expansion. Too much, perhaps, has been claimed for that principle, and too little granted, by the controversialists. It is now shewn (column 21) in what ratio over other engines, the efficiency of steam is increased, by the Cornish system of employing it, separate from all consideration of the greater or less work done by the coal, whether arising from peculiarities in the structure of the boilers, from the calorific strength of the coal, or other causes. We are not, however, justified in concluding that the great dynamic advantage, still resulting, is due to the single circumstance of that engine being worked by highly elastic steam, greatly expanded; other causes may contribute to produce the effect.

It is necessary, also, to guard against conclusions which might be deduced, from a comparison of the effects of the Cornish engines in the table, with the pressures on the piston and degrees of expansion, set down in columns 5 and 6. The pressures given were not ascertained by any instrument, (excepting at Huel Towan,) and must be considered only as estimations, not as facts. The pressure upon the piston during the interval which occurs between the first admission of steam into the cylinder, and the instant of shutting it off, may be very variable; that it was so, in several engines to which Mr. Henwood applied the indicator, is evident from the diagrams he has given, annexed to his paper. (Trans. Inst. C. E. Vol. II.) At the Huel Towan engine, when the steam in the boilers was at a pressure of 47.1 lbs. above the atmosphere, it varied from 12.3 lbs. to 7.3 lbs. per square inch on the piston, during its admission into the cylinder; which latter was its elastic force, at the instant of closing the steam-valve *. I adduce these facts, with the view of shewing the impossibility of determining the precise amount of pressure on the piston, from the degree of *wire-drawing* the steam; and as a caution against expectations of deducing any valid theory of the action of the steam, in these Cornish engines, from the particulars of pressure and expansion, contained in the Table, which are only approximations to the truth.

A few observations are necessary, with reference to the correctness of the weights of water as steam, assigned as the consumption of the Cornish engines. The Huel Towan engine was experimented upon by Mr. Henwood, with all due care, during 24 hours; the results of the United Mines experiments are drawn from a registration of water, fuel, and duty, during eight months of continuous work. In my paper on evaporation, (Trans. Inst. C. E. Vol. II. p. 175,) I noticed the very considerable differences in the monthly returns of water vaporized at the latter engine; which, however, I considered to be accounted for, by the statement given in the note to that page. The subsequent investigation of the boilers of these two engines, the near correspondence between their extents of surface exposed to heat, their near equality in the rate of combustion, &c., as shewn in the Table of boilers, justify the assumption, that the United Mines experiment is worthy of belief. As such I have introduced it, being in close agreement with Mr. Henwood's results; and I can see no more reason for doubting the one than the other. Mr. Hosking, the resident engineer at the United Mines, obligingly remeasured the boilers, and supplied me with all the particulars relative to them and to the engine, which I required to complete the Tables.

* These facts were communicated to me by Mr. Henwood.

I have applied the mean of evaporation obtained by a pound of coal from the boilers of the above named engines, as data for the determination of the consumption of water as steam, in two others; viz., the one at Holmbush, (Experiment VIII.), whose performance and fuel were so accurately ascertained by Mr. Wicksteed; and of the Fowey Consols engine, (Experiment IX.,) which formed the subject of the celebrated trial in 1835.

Knowing, as we now do, the product of steam from a given surface of the Cornish boilers; knowing also, that the Cornish engineers habitually allow that, or a nearly corresponding, extent of surface, for the production of similar effects; we are as much justified in assuming a similar performance to result, in all cases, as we know would take place, if an engine of the same dimensions as Watt's, at the Albion Mills, with equal power of boilers, were experimented upon.

Of the amount of duty at Holmbush, there cannot exist a doubt; the water raised was weighed, and measured. Mr. Wicksteed's experiment thereby establishes the general accuracy of the Cornish statements of the duty done by their engines; and, as the duty of the Fowey Consols was the subject of a most patient and careful trial, during 24½ hours, by a committee of perfectly competent persons, I regard it—though exceeding in amount all previous and subsequent trials—as authentic as any other on record.

As the evaporation was not ascertained at either of those engines, a question might be started, whether their additional duty, compared with the United Mines and Huel Towan engines, may not have arisen from a superior evaporation per unit of coal, rather than from a superior dynamic effect, obtained from the steam generated. In the case of the Fowey Consols, I have satisfied myself, by enquiries of Mr. West, the maker of the engine, that the increased performance was due to the better use of the steam in the engine, not to any greater evaporative product from those particular boilers. Mr. West states that during the trial in 1835, the evaporation was 100 gallons from the temperature of the hot-well, by the bushel of 94 lbs., that is 10.63 lbs. of water vaporized by 1 lb. of coal. In reply to a second, and more recent enquiry, Mr. West writes, as follows:

"I have made another trial, for the purpose of ascertaining the quantity of water evaporated, by the consumption of a bushel of coals, (94 lbs.,) and find it come very near my former experiment. The trial was as follows. The feed plunger-pole and its stroke were accurately measured; the number of strokes made in 24 hours amounted to 1920; the diameter of the pole is 6 inches; the

stroke 2 feet 6 inches, being equal to a little more than 3 gallons per stroke, or 5873 gallons in the 24 hours, in which time 60 bushels of coals were consumed, making 98 gallons to a bushel. The temperature of the water in the hot-well was 85°, and on entering the boilers from the warming tube, about 180°. The pump and boilers, at the time I made the experiment, were in good order. The full length of the stroke of the feed-pump is 2 feet 7 inches, from which I have deducted one inch, to allow for waste, &c.

" When our engine at Fowey Consols was on trial, the steam, on entering the cylinder was about 27 lbs. pressure on each square inch, and cut off at ¼ stroke."

This second experiment gives 10.41 lbs. as the evaporation by 1 lb. of coals; and the mean of the two is 10.52 lbs. The mean of the two experiments at the Huel Towan and United Mines, is 10.51 lbs. The evaporative results are, therefore, identical *, and the difference between the duty of the engines, is attributable, only, to the difference in their respective expenditure of power, to produce equal effect; or, in other words, to their respective dynamic efficiency.

Experiment IV., made by myself at Warwick, on my own engine of Boulton and Watt's construction, is the result of observations continued for several months, and is strongly corroborative of the results obtained at the Albion Mills.

In the conditions of the two experiments, there are, however, some points of importance to notice. It is requisite to distinguish between the nature of the loads on the two engines; the Albion Mills engine, grinding corn; the Warwick engine, driving worsted spinning machinery. The load of the first was, consequently, by far the most uniform of the two. My experiment includes the waste, and loss of steam, attendant on occasional short stoppages; to which all manufactories are liable. The engine was fully loaded, but, like all manufacturing engines, its load was subject to continual, almost to momentary, fluctuations. It had, generally, to be started, with such a load upon it, three times a day, as to require *blowing through* each time—an operation which swallows much steam—but no estimate of that useless consumption could be formed.

The fuel, at the Albion Mills, was that expended, simply, in supplying 10 hours steam to the engine. The fuel, at Warwick, worked the engine during 11½ hours each day, and includes the loss to maintain the boilers hot, during

* The correspondence is the more satisfactory, as the experiments were made at different engines, at different periods, by different persons, and by different methods.

$12\frac{1}{2}$ idle hours, out of the 24 hours; but, as no steam was ever permitted to blow away through the safety valves, the consumption of water as steam, which did the work, is absolutely correct.

At the period of my entering on these experiments, I was unacquainted with the indicator; but that instrument was, in the course of them, applied to the engine by Mr. Creighton, of Soho; and its load, with all the machinery at work, was determined by him to be 26 horses power. The mean load was, probably, somewhat less than this, but it is impossible to say by how much; nor can a nearer approach to accuracy be obtained from any engine, working under similar circumstances. It is a specimen of the *practical* expenditure of power and fuel, in a manufacturing engine; the nature of which does not, however, admit of sufficiently exact results to justify *theoretical* deductions. The pressure of steam on the piston, sometimes exceeded, and sometimes fell short of the pressure of the atmosphere; and I have called it equal in column 6. Deducting $\frac{1}{10}$th for the loss of steam at each stroke, arising from the steam passages, jacket, and other waste, the ratio of the volume of steam consumed, to that of the water which produced it, comes out as 1720 to 1; shewing the force of steam upon the piston to have about balanced the atmosphere.

The daily and hourly registration of the water as steam which passed through the cylinder, gave me an insight into the exact state of the engine, and its load, at all times. The practical value of this knowledge can scarcely be overrated. The engine was an old one; the piston packed with hemp; the cylinder a good deal worn; but we were warned of any increase in the expenditure of steam, by the hourly consumption of water; so that a defect was searched for, and a remedy applied with the least possible loss of steam. Though the mean consumption was 70 lbs. of water per horse power, per hour, I have known 80 lbs. consumed when the packing was worn, or the vacuum less perfect than usual; and I was never able to reduce the consumption of water lower than 65 lbs. per horse, per hour, for many hours successively.

I once made an experiment to ascertain, during a whole day, the consumption of steam by the engine, working idle—and, afterwards, with the whole machinery, working idle, i. e. unclothed with wool—with the view of arriving at a knowledge of the real power consumed by the processes of the manufactory; but, unfortunately, I either did not note down the results, or have lost them. Such experiments would be very valuable to manufacturers; for, though the indicator informs us of the pressure on the piston, it does not disclose the expenditure of power; the registration of the water, alone, unfolds this know-

ledge. It checks, and tests the industry and skill of the attendant engineer; it informs him of the state of his boilers, of his engine, or of that of its load, which, from carelessness, is often greatly increased for want of oil, &c.; and it instructs the master as to the numerous causes of waste, and the expensive consequences of negligence.

In comparing the results of this engine with those of the Albion Mills, it would appear, from the duty tested by the fuel, that the former had the advantage; whence, it would be concluded, that the Warwick was the better engine of the two, but when brought to the true test of the *efficiency of the steam* in each, it is seen that the Albion Mills engine had a slight preponderance in its favour. The evaporative superiority was obtained in the Warwick, through the careful nursing and system of management explained in my paper on Evaporation, and at pp. 12 and 13 on the Boilers.

Experiments II. and III., on two high-pressure non-condensing engines, were, also, made by myself. I attached great importance to both these experiments, and conducted them with all the care of which I was capable, being unacquainted with any practical determinations, by engineers, of the consumption of steam by this class of engine.

Experiment II. The engine drove a silk mill, and being employed by its owner to reset the boilers, I determined, at the same time that I ascertained the amount of evaporation, previous to altering the boilers, to ascertain, also, the load on the engine. I used the indicator, and continued the experiments during three successive days; the mean of these is set down in the Table. The water was injected into the boilers from a measured vessel;—the valves, and piston were perfectly tight, but the cylinder was not jacketed; and as the valves were only single, (set half way between the top and bottom of the cylinder,) I attribute the excess in the consumption of water as steam by this engine over the next related experiment, to the greater quantity of ineffective steam wasted in the passages, and to condensation in the cylinder, for want of a jacket.

Experiment III. This engine drove bark mills, and other machines. I was engaged to arbitrate a dispute between the maker, and owner of the engine, respecting its power. I found the engine perfectly well made; the cylinder jacketed, and clothed outside; the valves double, with no extraneous spaces; the piston had metallic packings; but the boilers were insufficient in surface, and ill set. Evaporative economy was, consequently, low. I had no

indicator, and used the mercurial column, with a stop cock, to determine the pressure on the piston. The Table gives the mean of numerous trials, which varied but little from each other. There was, originally, a loss of power in this engine, arising from too contracted eduction pipes; these were enlarged, so as to relieve the piston from any counter resistance beyond that of the atmosphere; and the results stated, were obtained after this alteration.

In both experiments, I ascertained that the boilers did not prime, (of which I had some suspicion from their small dimensions,) by testing the evaporation, with the man-hole covers removed, which corresponded very nearly with the product from equal weights of coal, burnt at the same rate as when the engines were at work.

The comparison of the duty done by 1 lb. of coal, and 1 lb. of water, as steam, (columns 10 and 11,) shews that the respective efficiency of these two engines, would be incorrectly estimated by the fuel consumed. The expenditure of power, for equal effects, by each, is seen truly in columns 11 and 16. The near coincidence between the results of these two trials is satisfactory; and, I think, conclusive as to the expenditure of power by this class of engine, though it is somewhat less in amount than I have commonly heard assigned to it. I am acquainted with a mine engine, which was worked either on the condensing, or non-condensing principle, according to the supply of water; and the coal accounts shewed that, in the latter state, when using steam at about 15 lbs. pressure, it consumed about twice as much fuel as when condensing. I have heard similar statements from several respectable quarters, and think it very probable that such would be the approximate results, from engines so converted; but experiments of this nature are, evidently, inadequate to determine the true relation of the efficiency of steam on the two systems.

The Table thus comprises determinations of the positive, and comparative excellence of the three great classes into which steam engines may be divided. Of these there are varieties, which must partake, in a greater or lesser degree, of the advantages of each system.

The Cornish engineers have recently extended the application of their system to rotative purposes, by engrafting rotation on single, as well as double-acting engines; and they have already attained the high duty of 60 millions and upwards *—a convincing proof that the benefit to be derived from the use of greatly expanded steam, is not limited to pumping engines. Marine en-

* Lean's Monthly Reports.

gineers are slowly following in the wake of the Cornish; prudentially, too, as there are many other considerations of importance besides economy of power, in a steam-vessel; but there can be no doubt that diminished expenditure of power, and, consequently, of fuel, will accompany the closer imitation of the Cornish practice. The combination of two engines, and the momentum of a steam-vessel, are elements, *per se,* particularly favourable to the application of this system in its fullest extent, to the purposes of marine locomotion; but the extent, to which it can be practically adopted, is a question to be settled only by experience, carefully, and gradually acquired.

It is a question, also, whether the extreme use of the Cornish system is suitable for those manufacturing engines, one great and essential quality of which is uniformity of motion. A steady velocity in the motive power, is of such consequence in cotton spinning, and several other of the arts, that any loss of it would be dearly bought by economic gain. The momentum of machinery of this description, is but trifling; and an equivalent must be found for it, in order to obtain the whole value of the Cornish expansive system. With the degree of uniformity of motion, attained by expansive rotative engines in Cornwall, I am unacquainted; this should be a matter of investigation by persons desirous of availing themselves of their economy, for uses which require such precise, and unvarying speed.

The continuous working of a steam-engine is productive of a large saving in fuel. A residence of some years in Manchester enabled me to inform myself of the average consumption of coal, by the best engines in that town and neighbourhood; which, being rated at about $1\frac{1}{2}$ hundred weight, per horse-power, per diem, or 15 lbs. per horse, per hour, may be considered as below, rather than above the mean. Of this quantity, it is estimated that one third is wasted in raising the steam each morning—in the loss occasioned by meal-time stoppages—and in the consumption of the night. Much of this waste is, doubtless, owing to careless management, to the neglect of covering boilers, &c.; such is, however, the expenditure of fuel, in present practice, in Lancashire. A Cornish engine, when doing an 80 million duty—a performance lower than Cornish engineers would guarantee—accomplishes the same work as a Manchester engine, with one sixth the expenditure of fuel. It has been suggested, that fully three-fourths of the entire effect of a Cornish engine would be realized, and the most uniform of motions obtained, by employing this engine to raise water upon a wheel, and thus transfer its power to machinery. The engine might then be

kept continually at work, and the horse-power would be obtained by the consumption of about 3 pounds of coal per hour.

This would, probably, be the cheapest of all means of obtaining power, for stationary purposes on railways, where fixed engines must lose, by waste, a very much larger proportion of steam and fuel than manufacturing engines, owing to the greater irregularity of the demand upon them. The pumping engine may be kept at work without intermission; its rate may be accurately accommodated to immediate, or anticipated wants; and the use of a reservoir, of very moderate dimensions, would, at all times, secure a supply of power, available at a moment's warning, by night or day. These arrangements are practicable in all situations where a sufficiency of water, for injection, exists; little waste would arise from the wheel, its water being continually returned to it, and that waste could be made good from the water discharged by the air-pump.

OF THE STANDARD MEASURE OF DUTY.

The present system of rating the duty of steam-engines in Cornwall, by the heaped imperial bushel measure, of coal, is open to great objections.

If it be necessary, from long usage, to purchase coals by measure, they should be weighed to the engine; as the calorific value of coal does not depend on bulk, but on the purity, and quantity of its carbon. This objection to measure, has, to a certain extent, been appreciated, and it has been found requisite to fix the *duty weight* of a bushel. It appears to me that as the weight of a bushel of coals is so various, and as it has been found needful to reduce these varying bushels to some common weight—94 pounds being the present Cornish standard of comparison in the Monthly Reports of duty—it would be better to adopt some multiple of a ton for that purpose; or, still better, to estimate the duty, by the effect of a single pound of coal. The difference in weight, of the bushel of coals, from mines in the same district, and different districts, is greater, perhaps, than is commonly imagined. From the published Minutes of Proceedings of the sessions of the Institution*, it appears that the bushel of coals has been ascertained by several engineers, to vary from 80 to 112 lbs.; and Mr. Henwood found, at three engines, the respective weights of the bushel of coals in use, to be 100, $92\frac{6}{10}$, and $88\frac{3}{10}$ lbs. (Trans. Inst. C. E. Vol. II.)

Fuel, unaccompanied by a statement of its evaporative product at each en-

* See Minutes Inst. C. E., Session 1838, pp. 3 and 7.

gine is, at best, but an imperfect standard of the merit of engines. A knowledge of its consumption is a necessary, and important item in accounts of expense, as it determines the money cost of working an engine; and we have seen the success which has attended the unwearied and skilful efforts to diminish it; but a more accurate standard of dynamic efficiency is requisite. It is possible that coal may not always continue to be the sole combustible employed to generate the power of engines; it may be exchanged for anthracite, coke, peat—substances already in partial use—or steam may be generated by artificial fuels, or by processes now unknown, and unimagined. The work done by any given weight of water as steam, is a sure index of the quality of a steam-engine; it is a measure unaffected by variable calorific agents; and, so long as the engine continues to be worked by steam, so long will the performance of different engines be accurately gauged by their respective expenditure of water as steam.

I will venture to suggest the use of the *pound of water as steam*, as the most convenient and correct standard of duty which can be adopted. In Table VI., column 11, this standard is applied to all the engines examined. Cornish engineers will perceive the ease with which the tyranny of custom would be shaken off, and the facility with which old associations would be melted into a new one, by the adoption of this standard. It happens—from the circumstance that the Cornish evaporation, in the best conditioned engines, amounts to $10\frac{1}{2}$ pounds, for each pound of coal burnt; and, from the circumstance, that the adopted weight of a bushel is but little short of 100 pounds—that the duty done by a pound of steam is nearly the one thousandth part of that done by a bushel of coal. The transition, therefore, would be easy and natural from millions, raised by a bushel of coal, to thousands of pounds, raised by one pound of water as steam. The image of effect would, also, be much more clearly impressed upon the mind by reference to this measure of force, rather than to the force obtained from a bushel of coals. Every one can understand and appreciate the weight of a pound; as well as the degree of force he would have to exert in raising that pound a foot in height; and, being informed that a pound of water, converted into steam, is capable—through the medium of the engine—of elevating one hundred and twenty-six thousand times its own weight, to an equal height, the mind would receive, not only a distinct image of the vast force of steam, and of the account to which it is turned by the ingenuity of man,

but it would imbibe an accurate idea of the true measure of the power of the steam-engine.

Great additional value would be conferred on the monthly reports of the Cornish engines, by incorporating in them two such columns as 10 and 11 in the table. The evaporation from the boilers of each engine might be stated separately, or it might be omitted; as the weight of water vaporized by a pound of coal, in each case, would be found by dividing the duty obtained from the pound of coal, by that obtained from the pound of water as steam.

In column 8, I have exhibited the duty equivalent to the use of one hundred weight of coal; but, though a multiple of the ton, I think the hundred weight would be objectionable as a standard, from its raising, still higher than the bushel, the expression of the load overcome. The millions of pounds, vanquished by the bushel, have already sufficiently tried the ingenuity of the expounders of the steam-engine, by inducing them to convert weight of duty into bulk; or to involve in it, as they have sometimes done, strange conceits of time and space, in order to represent, as they imagine, a less complex notion of this simple proposition.

OF THE CONSTITUENT HEAT OF STEAM.

The weight of water, as steam, would not be a true measure of the relative consumption of heat by different engines—which is the economical result sought—if a given weight of water, as steam, at different elasticities, contained variable amounts of caloric. Experiments, to determine the constituent heat of steam, conducted with great care, by various methods, and leaving little to be wished for, have been made by several philosophers. Their conclusion, that the sum of heat in steam, at all specific gravities, is a constant quantity, is generally received as a law of nature.

It occurred to me that it would be practicable to perform a set of experiments, within certain limits, which would have the effect of confirming, or disproving this assumed fact, by ascertaining the quantity of water which a given weight of coal would convert into steam, at various pressures. It was requisite that these experiments should be made on the same boiler, with the same kind of coal, and that an equal weight of coal should be consumed, during each experiment, as nearly as possible in the same time; or that equal quanti-

ties of water should be evaporated, under the different pressures, as nearly as possible in equal times.

The following Table gives the result of 28 experiments, made during 28 successive days, on vaporization from the boiling point, to 60 pounds pressure per square inch above the atmosphere. They are presented, as essentially confirming the doctrine first made known by Watt and Southern; as practically conclusive that there is no material difference in the consumption of fuel, to convert equal weights of water into steam, of the elasticities cited; and, consequently, that the relative efficiency of steam in engines is solely due to the manner of using it, not to any change in its chemical constitution at different pressures.

Table VII.

Number of experiments.	Pressure above the atmosphere.	Temperature of the steam.	Total weight of coal burnt.	Mean weight of coal burnt, per experiment.	Evaporation in cubic feet.	Mean duration of experiment.	
No.	lbs. per sq. in.	Temp.	lbs.	lbs.	Cub. feet.	Ho.	min.
4	0	212°	800	200	20	10	
1	5	226.30	199	199	20	9	55
1	10	237.64	202	202	20	10	1
3	15	247.94	585	195	20	9	50
2	20	256.78	396	198	20	10	2
1	25	264.82	204	204	20	10	4
1	30	272.02	200	200	20	10	
1	35	278.80	203	203	20	9	58
2	40	285.04	202	202	20	9	59
2	45	290.76	408	204	20	10	5
3	50	295.96	615	205	20	10	
3	55	300.76	624	208	20	9	57
4	60	305.06	840	210	20	10	2

I will now proceed to relate the manner of conducting these experiments, as their value entirely depends on the precautions taken to ensure accurate results.

The boiler employed, was, in its exterior form, precisely similar to an ordinary locomotive one; but of different dimensions and internal structure. The fire-box, inside, was 4 feet square, and the grate, consequently, 16 square feet in area. The heat then passed through five tubes, 10 inches diameter inside, and 5½ feet in length, contracted at the smoke box end to 5 inches diameter, in the last 12 inches of their length. The chimney was twenty feet high from the ground, and 15 inches diameter, with a damper in it near the summit. In the first

place, I ascertained how much water the boiler would hold, in the visible length of the glass gauge, so as not to leave the top of the fire-box uncovered, nor yet to rise so high as to endanger the accuracy of the experiments by priming. The water was both weighed and measured into the boiler, by imperial standards; a scale was adjusted to the glass gauge, which sensibly denoted a cubic foot, and admitted of subdivisions; and I found I had 20 cubic feet of water to operate upon. The capacity, thus ascertained and graduated, served for all the experiments. This being done, I ascertained the weight of coal, which would, probably, be required to evaporate the 20 cubic feet of water; but, as I wished to render the combustion very perfect, and ensure, as far as possible, that no uncombined air should pass through the fire, I diminished the area of the grate one half by bricking it over, air-tight, and left the air spaces, between the bars, $\frac{3}{16}$ths of an inch wide, the bars being $\frac{3}{4}$ of an inch in breadth.

As the experiments in contemplation would last many days, and it being essential that the coals should be as nearly uniform in purity and strength as possible, I went to a neighbouring pit to procure them; and into the pit, that I might assure myself of every lump coming from the same vein, and the same part of it. I thus procured 3 tons, and had every lump broken into pieces which would pass through an inch and a half ring; rejecting every piece which exhibited the slightest symptom of impurity. The coal was then carefully covered up from rain or dampness.

It may not be uninteresting here to state, that I tested the coal as to the moisture it contained, when raised from the pit, and found it to be about $\frac{1}{20}$th of its weight. As a measure of precaution, also, I each day weighed 1000 grains of the coal submitted to combustion, dried it thoroughly in a warm oven, and ascertained its loss, which varied from 4 to 6 per cent., during the 28 days of the experiments; so that no change took place in the humid contents of the coal.

A thermometric steam-gauge, graduated for temperatures and pressures, (made by Mr. Adie, optician, Liverpool,) was applied to the boiler, without which I should have been unable to have kept the fire, and rate of evaporation, so steady as I desired. The safety-valve was similar to those commonly used on locomotive-engine boilers, and had been carefully weighed, when the degrees of pressure were marked on its spring balance. During these experiments, the spring balance was released from its hook, and weights were suspended to it, that it might rise freely at the blowing point of each experiment. The scale of the

valve was graduated from 10 lbs. to 65 lbs. above the atmosphere. It rose very evenly, throughout the whole range, in correspondence with the pressures marked on the thermometric gauge, excepting that it was uniformly somewhat later in its indications than the former. This is explained by Mr. Adie, (with whom I communicated on the subject,) by his stating that the temperatures, and corresponding pressures, marked on his instrument, were from 2 to 4 degrees of Fahrenheit below those determined by the French academicians, as they were not corrected for the portion of the mercury exposed to the cooling of the atmosphere—a difference which was to be expected. The correspondence between this thermometer (with the correction supplied) and the safety-valve, were thus a proof—though a rude one—of the accuracy of the determinations of the French philosophers. Mr. Adie's graduations were from experiments, made by himself, with steam from the boiler of the locomotive engine, Venus, brought to a mercurial column, on the Liverpool and Manchester Railway.

These arrangements being made, a few preliminary experiments shewed that about 200 lbs. of coal would be required, and that about 10 hours would be the time occupied in vaporizing the 20 cubic feet of water, at the slow rate of combustion, which I thought best fitted to give correct results. As the damper did not fit quite close enough, to restrain the fire sufficiently, I diminished the aperture of the five tubes, at the point of exit into the smoke-box, by one half.

The experiments were prosecuted as follows.—Each morning 200 lbs. of coal were weighed by myself, the boiler having been previously pumped somewhat fuller than the level of commencement, and the steam raised to the pressure of the intended experiment. When the thermometer shewed signs of falling below that pressure, and the fire was unable to elevate it, the water was drawn off to the zero or upper division of the glass gauge; 185 lbs. of coal were immediately put lightly and equally over the whole surface of the grate, and the fire door instantly plastered round with clay. As the draught and combustion were very slow, it required about half an hour before evaporation commenced, and the thermometer usually fell 2 or 3 degrees during the first quarter of an hour; until the coals became a little ignited. The fire door was opened at the sixth hour to examine and level the fuel; and as the evaporation, which was noted every quarter of an hour, was nearly alike each day and each hour, it became easy to judge how much more coal would be required; and my practice was to add 10 lbs. at that hour; clay up the fire door again; and, as soon as the steam began to fall, to add as much more coal

as would complete the evaporation of the 20 cubic feet. The duration of each experiment is reckoned from the moment of the rising of the valve, at the respective pressures, till it closed from the exhaustion of the fire.

At the termination of each day's experiment, the boiler was well cleansed by blowing off, through several cocks, when the steam was yet up, but the fire extinguished. The fire-box, grate, and flues were swept free from all deposit or attached soot, and the boiler replenished with water for the following day's experiment. I attended each day at the starting of the experiment, and until the regular rate of evaporation exhibited itself. I visited the boiler two or three times during the first six hours, and was always present from the sixth hour till the conclusion. When any thing occurred to cause me to doubt the accuracy of an experiment, it was repeated on the ensuing day; and though, in reality, every one of the experiments is entitled to as much confidence as another, it will be seen that as I approached the higher pressures, I made more experiments on each.

The columns, mean weight of coal, and time, express the means of the number of the experiments on the same pressure; the variation in the weight of coals burnt, did not, however, exceed 4 lbs., nor the time more than five minutes, in any of these, so that I have thought it unnecessary to encumber the table by inserting other than the mean time.

I am confident that not an ounce of water was wasted any one day. I wished to have experimented at 65 lbs., but one of the rivets wept a little at that pressure, and I abandoned it.

It will be remarked that, from the commencement to the termination of each day's experiment, my assistant had nothing to do but to observe his thermometer, note the rate of evaporation, regulate his damper, and occasionally hand the safety-valve to maintain the pressure of steam uniform. The thermometer indicated, with great delicacy, when the damper required opening or shutting, by a single notch. No water had to be pumped; the fire had not to be stirred; nor could any thing occur to derange the observations.

I, therefore, consider these experiments as substantively conclusive, for all practical purposes, that equal quantities of heat enter into the composition of equal quantities of water in the shape of steam, at all the temperatures and pressures noted; and, consequently, that the sum of the sensible and latent heat, in steam of the specific gravities belonging to these pressures, is a constant quantity, as previously determined by nicer experiments. The greatest difference in the results

of any two experiments is $5\frac{1}{2}$ per cent., and I feel no hesitation in expressing my own conviction, that the small increase in the consumption of coal observable at the more elevated pressures, was simply owing to the circumstance, that, in consequence of the increased temperature of the absorbing water, at those pressures, the heat necessarily quitted the boiler at a somewhat higher temperature than at the lower elasticities.

I omitted to state that, in the first two of the four experiments at the boiling point, the man-hole cover and safety-valve were removed, and two cocks opened, that the steam should have the freest possible exit, in order to confirm my faith in no unvaporized water passing over with the steam in the subsequent experiments. In the two others, stated as from 212°, the whole of the steam escaped through the safety valve aperture, exhibiting an elevation of about two degrees of temperature. At 5 lbs. on the inch, the safety valve was replaced, and regulated by hand, as its own weight, with the lever, spring-balance, and rod, equalled 10 lbs.

It may be said, and with truth, that though equal volumes of water were evaporated, the weights were not identical, owing to the dilatation of the water for its temperature; but this would cause so slight a difference, that I preferred the plan of evaporating equal bulks, previously heated in the boiler to the experimental degree, to that of pumping the supply during each experiment: for, in the 28 days, the water I was using would have varied nearly 15 degrees in temperature, on entering the boiler.

The experiments were made in January and February, 1837. The boiler was much exposed, being fixed upon a machine for agricultural purposes, then on a bog; but the whole being covered with a roof, no rain or snow entered, and the boiler was clothed with sacking and tarpaulins, so that the varying temperature of the atmosphere, out of doors, could have but little effect upon it. Every precaution was used to preserve, as nearly as possible, an uniformity of temperature about the boiler.

The coal was extracted from one of the pits belonging to Wm. Hulton, Esq., of Hulton, near Bolton, a coal well known in Lancashire for the purity and strength of its coke; and I took the opportunity of making four experiments on the latter combustible; viz. one at the boiling point, one at 20 lbs., one at 40 lbs., and one at 60 lbs. pressure. The consumption, respectively, was 148, 149, 152, and 153 pounds to evaporate the 20 cubic feet of water, in 10 hours; being about 25 per cent. less than the coal from which it was prepared. It must, however,

be observed, that, as the coke required more draught than the coal, I was obliged to widen the air spaces between the bars, and to open the ends of the five tubes of the boiler, so that a greater waste took place, of droppings into the ash-pit, and of heat through the chimney.

TO FACE PAGE 76.

TABLE VI.

No. of experiments.	DESCRIPTION OF ENGINE AND PLACE OF EXPERIMENT.	PARTICULARS OF THE ENGINES. Diameter of cylinder.	Length of stroke.	Number of strokes per minute.	Expansive or unexpansive.	Mean pressure on the pistons above or below the atmosphere.	Mean pressure in the boilers above the atmosphere.	DUTY. WEIGHT IN POUNDS RAISED ONE FOOT. By 112 lbs. of coal.	By 94 lbs. of coal (Cornish bushel.)	By 1 lb. of coal.	By 1 lb. of water as steam.	Effective power of the engines at the time of the experiment.	Consumption of coal per hour.	Consumption of water as steam per hour.	Consumption of coal and water as steam, per horse power per hour (33000 lbs. raised one foot per minute).		Evaporation in lbs. of water by 1 lb. of coal from the initial temperature of the water.	Heated surface of boiler per horse power	Heated surface of boiler for each lb. of coal burnt by one horse power per hour	Heated surface of boiler for each lb. of water as steam used by one horse power per hour	COMPARATIVE ECONOMICAL RESULTS. Efficiency of the steam, Albion Mills Engine, Unity	Efficiency of the fuel, Albion Mills Engine, Unity.	DATE OF EXPERIMENT, AND AUTHORITY.
		In.	Ft. In.	Strokes.		lbs per sq. in.	lbs per sq. in.					Horses power.	lbs.	lbs	Coal, lbs.	Water as steam, lbs.	lbs.	sq. ft.	sq. ft.	sq. ft.	Ratios.	Ratios.	
	ATMOSPHERIC ENGINE. Single—Pumping.																						
I.	Long Benton (Northumberland).	52	7.0	12.00	......	......	1.50	12600000	10575000	112500	14280	40.50	714.00	5625.00	17.63	138.88	7.87	11.33	26.03	3.305	0.501	0.489	Smeaton, 1772. (Farey on the Steam Engine, Vol. I.)
	NON-CONDENSING ENGINES. Double—Rotative.																						
II.	Congleton (Cheshire)	13	4.0	27.50	Unexpansive.	+20	25.00	12418560	10422720	110880	15840	12.00	214.28	1500.00	17.85	125.00	7.00	......	......	......	0.556	0.482	J. Parkes, 1823.
III.	Paris	20	5.6	20.00	Ditto.	+14	20.00	11088000	9306000	99000	16500	20.00	400.00	2400.00	20.00	120.00	6.00	......	......	......	0.579	0.437	J. Parkes, 1827.
	CONDENSING ENGINES. Double—Rotative.																						
IV.	Warwick	25	5.6	20.00	Ditto.	Equal to the atmosphere.	2.50	29056832	24386986	259436	28285	26.00	198.43	1820.00	7.63	70.00	9.17	17.50	59.63	6.500	0.992	1.127	J. Parkes, 1820.
V.	Albion Mills (London). . . .	34	8.0	16.00	Ditto.	Estimated at −2.50	2.50	25756752	21617274	229971	28489	50.00	432.50	3475.00	8.65	69.50	8.03	10.43	60.28	7.500	1.000	1.000	Watt, 1786. (Farey on the Steam Engine, Vol. I.)
	CORNISH CONDENSING ENGINES. Single—Pumping.				* Expansive. Steam cut off at	* Previous to expanding.																	
VI.	United Mines	85	10.0	5.57	$\frac{1}{7}$th stroke.	+16	35.00	80817399	67828888	721583	69097	104.75	287.50	3001.87	2.74	28.65	10.44	30.26	1156.93	114.112	2.425	3.137	Lean's Monthly Reports, 1837. (The duty assigned to this engine is the average for the same month as that of the evaporation from the boilers noted in my paper, Trans. Inst. C. E., Vol. II, p. 174. J. P.)
VII.	Huel Towan	80	10.0	5.35	$\frac{1}{5}$th ditto.	+ 7.3	49.40	96975200	81389900	865850	81732	89.06	203.78	2156.21	2.28	24.21	10.58	29.19	1144.03	107.390	2.868	3.765	Henwood, 1831. (Trans. Inst. C. E., Vol. II. p. 58.)
VIII.	Holmbush	50	9.1	4.63	$\frac{1}{6}$th ditto.	+30	40.00	140484848	117906992	1254329	119097	26.48	41.77	440.00	1.57	16.61	......	......	......	......	4.180	5.454	Wicksteed, 1836. (Trans. Inst. C. E., Vol. II. p. 63.)
IX.	Fowey Consols	80	10.4	4.29	$\frac{1}{4}$th ditto.	+27	40.75	149050160	125095713	1330805	126359	62.01	92.35	972.62	1.48	15.68	......	......	......	......	4.435	5.786	Mining Journal, Oct. 31, 1835, and published report of the trial; also Lean's Report on the Cornish Engines, compiled for the British Association for the Advancement of Science. 1839, p. 97.
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23

* Vide page 61.

Back of
Foldout
Not Imaged

OF THE LOCOMOTIVE ENGINE.

The locomotive, as an instrument of mechanical power, differs only by its use of the blast from the fixed non-condensing engine; it is the same engine set on wheels; it receives, and transfers the force of the steam, in a manner identically similar to a stationary engine; and its effects are ascertainable and measurable by processes, common, and applicable to each. In its locomotive character, it resembles the marine steam-engine; both engines form integral parts of the mass of matter set in motion by them; the power of each is communicated to the mass, by contrivances varying only in their adaptation to the media on which they operate; but, both engines are at rest, relatively to the mass moved, and to the media of motion. The power and effects of both engines are, therefore, recognizable, and appreciable by one or other of those methods of investigation previously described, and employed in practice, as they may best suit the special circumstances, work, and nature of different engines.

The method resorted to by experimenters on the locomotive, has been that of attempting to determine the amount of resistance opposed to its progress, in preference to that of ascertaining the power expended in overcoming the resistance. The solution of either of the two problems would be equivalent to the solution of both, it being an axiom in mechanics, that power and resistance are constantly in *equilibrio;* the amount of one of these forces, being ascertained would, consequently, be a true measure of the amount of the other.

The search after the sum of the resistance, by the process of analysing its several constituent quantities, has been attended with perplexing difficulties; as that sum arises out of forces, which, though distinct, are yet so numerous and variable, that the determination of the exact quantity assignable to each, at any one velocity, has hitherto eluded the sagacity and industry of all the experimenters who have explored the subject. That the investigation has failed of success, will, I think, be demonstrated by the following examination of such results and conclusions as the several labourers in this inviting and fertile field of experiment have presented to us, as facts; or which, as deductions from natural laws, ought, in their opinion, to command universal assent, and be received as a creed based on immutable principles. I think it will be apparent, from the indications of the tests to which I propose to submit these so-called facts, and deductions, that the first are but erroneous estimations, and the second, unwarranted, and untenable hypotheses.

The exposure of fallacies is far from being an agreeable task; but, if truth—fabled as being concealed at the bottom of a well—is to reward our researches, we must not be scared from its pursuit, by the weight of superincumbent matter to be removed, before the light shines on the object sought. I have laboured diligently, though humbly and less publicly than several other enquirers on this subject; I have carefully studied the exposition of their experiments and deductions; and, in discussing them, I trust that no difference of opinion with the authors, whose works it is necessary for me to examine, will be construed into any want of respect on my part for men whose talents, and persevering exertions entitle them to the esteem of all who seek to illustrate the practice, or principles of engineering science.

The first analyst, in the order of time, who applied himself to the solution of the several problems connected with the locomotive engine, was M. de Pambour. His work* appeared in 1836. I received it when engaged in nearly daily practice and experiment, with an engine precisely similar to the railway locomotive, though applied to a very different species of locomotion. The consumption of steam and fuel, by this engine, had necessarily occupied much of my attention, it being requisite to inform myself of the cost of power, in order to determine the important item of expense, for the purposes to which the engine was destined. I had experimented upon the additional resistance occasioned by the blast, at certain pressures; upon the expenditure of water as steam, at different loads, and at different velocities; and, from a previous knowledge of the consumption of steam, for given effects, by fixed high-pressure, and other engines, I felt to have in my hands a safe test, within certain limits, of the accuracy of my results. These will appear in their place in the sequel.

M. de Pambour's experiments arrested my earnest attention, and I immediately set to work to reduce them to the standards, by which the engines are compared in Table VI. In this there was no difficulty, as the tractive force was given; whence both the duty, and horses-power were deducible.

The results obtained from the very first investigation surprised me, it appearing that, if the estimate of resistance were correct, a duty had been accomplished, and a horse-power had been created, by a much smaller expenditure of water as steam, than was possible even by the condensing engine. I imagined, therefore, that some error had crept into my calculations; or, that some extraordinary economic principle belonged to the locomotive engine, which had

* A Practical Treatise on Locomotive Engines upon Railways.

escaped my notice; or, I was forced to conclude that the resistance assigned, viz. 8 lbs. per ton, was considerably greater than could have been overcome by the steam, at the velocity of the engine under examination. I repeated the calculations, repeated the experiments on the engine under my charge, and found the same discrepancy between M. de Pambour's and my own results. These calculations were made upon the Atlas, at the velocity of 9.72 miles per hour, being the first among the experiments given in the following Tables.

A more attentive study of this author's work, disclosed to me, also, that he had not ascertained any data by which to estimate the surplus resistance to the effective steam in a locomotive, over a fixed high-pressure engine, occasioned by the counter elasticity in the blast-pipe; the amount of which would reduce still lower, the consumption of water as steam, for a given effect. In addition, I found that the tractive force was considered by the author to be uniform at all velocities; inasmuch as he assumed the resistance to the progressive motion of the train, to be equal at all velocities. This was a puzzle; for, it seemed self-evident, since an engine could not drag so heavy a load at a high, as at a lower velocity—and, since it appeared, from the evaporative data, that a larger consumption of water as steam took place at the higher than at the lower velocities, in every instance—it seemed, I thought, self-evident that, rated on the ton of matter moved, the tractive force must have been, from some cause or other, increased at the higher velocities, in a proportion not greatly differing from the inverse ratio of the loads. Some great misconception appeared to me to pervade the whole of these experiments, for, if the deductions were erroneous in one case, they were so in all, being all based on data similarly obtained; but, whether the error arose from fallacious deductions, or inaccurate observations, it was out of my power to clear up the mystery. Strong doubts, too, arose in my mind as to the correctness of the evaporative data, my experience of locomotive engines having assured me that priming, in a greater or less degree, existed in all of them; and, by that amount, whatever it was, the water consumed as steam would have to be again diminished.

After making several abortive attempts to reconcile the results of these experiments with those on my own, and other engines, I determined to defer a more rigid scrutiny of them, to a future period, in the hope that M. de Pambour himself, or other experimenters, would throw additional light on the subject; that they would supply the necessary corrections; or renew the investigation on some fresh basis.

The examination, which my little leisure then permitted, led me to reflect on the fitness and accuracy of the method adopted by M. de Pambour of deducing the resistance to traction, from the laws of gravitation; and I was disposed to conclude that no certain results could come out of this method; that no practical data, worthy of confidence, as to the amount of resistance on a level, could be derived from observations on engines and trains moving down inclined planes; and that, if positive and appreciable weights could not be employed to determine the tractive force at various velocities, recourse must be had to some other measure of effect. No definite experiments had, at this time, been made with the intent to separate the resistance of the air, from the frictions of motion; nor to resolve several other items into which resistance and friction might be subdivided, and which enter, as distinct quantities, into the composition of the sum of the resistance. Further, I felt disposed to doubt the practicability of performing any set of experiments which could satisfactorily evaluate the ever fluctuating, and inconstant quantities of the various frictions, and resistances, to which a locomotive engine is subject, from the peculiar character of railway traffic.

These opinions stimulated me to endeavour to discover some certain criterion of the mechanical effect produced by a locomotive engine, at all velocities; some criterion which would apply as practically, as appropriately, and as distinctively to locomotive, as *duty* to pumping, and *horse-power* to rotative engines; and, thereby, enable us to draw a rigorous comparison between the increments of resistance, and the increments of velocity. If the work of a locomotive furnished a measure of this nature, it seemed to me that several problems of great practical consequence might be solved; and I considered it of far less importance to be able to distinguish (were it possible) the precise value of each particular unit of resistance, than to determine, correctly, the relative sum of resistance, and the relative expenditure of power, at all velocities, and under all circumstances.

This study opened to me a glimpse of a method of comparing the performance of railway engines, which will be treated of in a separate section.—To perfect, and prove its application and powers, additional experiments to those of M. de Pambour were requisite; and it was necessary to await the gradual development of the railway system, by the opening of new lines, and the publication of experiments on locomotives which the extended use of them would, doubtless, occasion. A sufficient number of these has accumulated, and I shall

first proceed to examine their results, according to the opinions and deductions of their authors, in the order of the dates of their publicity. They consist of the labours of M. de Pambour, Mr. Robert Stephenson, Mr. Nicholas Wood, and Dr. Lardner.

Experiments by M. DE PAMBOUR.

M. de Pambour's celebrity as a railway analyst—the adoption of his views and conclusions by a large class of persons—the later rejection of them, or substitution of still higher values of resistance by another class, equally confident in the sufficiency of their experiments, and in the truthfulness of their deductions—demand that his experiments should undergo a very minute and rigid investigation. It is the more fitting, too, that I should examine these experiments with the utmost care, as my own conclusions (so opposed as I have already explained them to be to those of the author) are based upon the comparison of the data he has supplied, with the ascertained, indisputable facts regarding the effects of other engines exhibited in Table VI. For this purpose it has been necessary to reduce the whole of his data to terms which permit the particular and comparative values of the power and effect to be shewn, when tested by similar measures. Tables VIII. IX. X., in which these results are classified, contain also those obtained from the data of other experimenters.

Table VIII. exhibits the particulars of the engines, and all the quantities required for ascertaining the duty accomplished at the resistance assigned to the load in each case, which is separated into the gross and useful duty obtained by the consumption of a pound of water as steam, and a pound of coke, respectively. A few words only of explanation are requisite on this head.

The work done by a locomotive engine is clearly a *duty*, in the strict sense of the term. Whether an engine be considered as dragging a load whose resistance is 8 lbs. or any other number of pounds, per ton; or, whether a weight of 8 lbs. for each ton of matter moved, descending over a pulley and attached to the load, be considered as the motive force, the result is the same; and that resistance—which is the real load on the engine—being ascertained, the whole duty, or effect, is found by multiplying this sum by the distance travelled in feet; and the product divided by the number of pounds weight of water and coke consumed, respectively, gives the expression in those terms. In estimating duty, the friction of the engine is disregarded; its amount formed a subject of distinct investigation by M. de Pambour, and is not included in his estimate of the resistance at 8 lbs. per ton; nor does it enter into the duty calculated for the other engines in the Table. The locomotive, and its tender are

here considered only as carriages, and their resistance forms part of the gross duty, as the steam overcomes it all; the useful duty is deduced from the tractive force required by the useful load, or weight of wagons and carriages, without the engine and tender.—Thus, supposing the following facts to be accurately ascertained; viz.,

1. The tractive force per ton of matter in motion;
2. The space passed over in feet;
3. The consumption of water as steam, during the trip;
4. The consumption of coke . . . . ditto;

we have all the elements given to find the duty, gross, or useful, performed by the steam, and coke. One example of the computation will suffice for all; vide Atlas, Exp. I.

Resistance, or tractive force, 1655.20 lbs. $\times$ 155,760 feet (in $29\frac{1}{2}$ miles) $=$ 257,813,952 lbs. raised one foot; which, $\div$ 8260 lbs. water consumed as steam $=$ 31,212 pounds raised one foot, by one pound of water as steam.

Table IX. contains the requisite data for computing the pressure against the pistons deduced from the sum of the resistances, first calculated on the assumed resistance overcome at the velocity of the wheel, in each experiment; and, also, the pressure on the pistons deduced from the ratio of the volumes of steam and water consumed. So far as this table relates to M. de Pambour's experiments on the resistance overcome, the pressure on the pistons of the several engines is calculated from the amount of the special frictions and resistances assigned by him. He observes, in the work referred to, page 239, "*that all the power applied is to be traced in the effect produced, and there is not a single pound of which the use may not be pointed out.*" I have already stated that the author assumed the resistance to progressive motion to be a constant quantity at all velocities; and that an important item of resistance to the effective power of the steam, had not been analysed by him, viz. the pressure against the piston, above the atmosphere, from the blast.

The pressure deduced from the sum of the resistances, given in column 29, for M. de Pambour's experiments I. to X., is composed, 1st, of the friction of the engine without load, which includes the resistance opposed to it as a carriage, in common with the train; 2dly, of the additional friction brought upon the engine by the load; 3dly, of the resistance of the load, at 8 lbs. per ton. According to the author, these three items include all the resistance overcome by the steam, excepting that occasioned by the blast, in excess above the pressure of the atmosphere.

The amount of the latter should, therefore, be ascertainable by comparing the whole force exerted by the steam on the piston, with the force assigned as requisite to overcome the aforesaid three, out of the four component parts of the total resistance. The difference between these pressures should represent the precise amount of the counter elasticity of the steam in the blast-pipe; and, thus, if the data were correct, "all the power would be traced in the effect produced, and the use of every single pound of steam pressure on the piston would be accounted for." To find the value of this latter force in each experiment, it is, therefore, only necessary to discover the total pressure which has acted on the pistons, and to deduct from it the pressure required to balance the ascertained resistances; and it is evident that their difference would be the unknown quantity—or blast pressure.

To arrive at this result, I have calculated, for each case, the volumes of water, and steam, consumed in equal times; and I have used M. de Pambour's own table for finding the elasticities corresponding with the respective ratios of the volume of steam to that of the water*. The total pressure of the steam upon the pistons; the pressure due to the ascertained resistances; and the resulting difference, are accordingly shewn in columns 29, 30, and 31; which, with the known pressure in the boiler, will be found useful tests of the accuracy, or inaccuracy, of the author's data. The application of the same tests to the other experiments, will be attended with equally searching effects, as to their exactness. (*See Table at the end of this Paper.*)

In order that no question may arise as to the fidelity of the sums set down in column 29, I have brought out from M. de Pambour's work, and given at foot, an example of the computation, using his own data and formulæ†.

In explanation of the sums assigned to the volumes of water, and steam

* The Theory of the Steam Engine by Le Comte de Pambour, page 76. 1839.

			Tons.	lbs.	
† Atlas. Exp. I.	Weight of engine		11.4	152	friction without load.
	,,	tender	5.5		
	,,	load	190.0		
			195.5 × 8	1564 lbs.	resistance of train.
				195.5	additional friction of engine owing to the load.

Sum of resistance, at the velocity of the working wheel 1911.5

1911·5 lbs. × 5·887 ratio of the velocity of the wheel and of the piston = 11,253 lbs. re-

consumed, it is necessary to state that the whole quantity of water evaporated (as given in column 11) is reduced, first by one-fourth, in conformity with M. de Pambour's own instructions, (pages 182 to 184,) as he found that, "of all the steam generated, a quarter was lost through the safety valves"; and the remainder is diminished by $\frac{1}{20}$th, being the proportion of ineffective steam, or the quantity wasted, at each stroke, by filling the passages between the valves and cylinders.

The volume of effective steam, therefore, (column 26,) being found by the multiplication of the capacity of the two cylinders, into the number of double strokes of the piston per minute, (columns 24 and 25,) and divided by the volume of water, corrected as above, (column 27,) the ratio which they bear to each other is ascertained (column 28); and the pressures corresponding with these ratios are obtained from M. de Pambour's Table above cited. One example will serve to illustrate the process of computation; Atlas, Expt. I.

sistance produced on the piston; which, $\div$ 226 area of the two pistons in inches = 49·77 lbs. resistance on each square inch of the surface of the pistons.

The following is the computation of the total resistance, and pressure on the pistons, for each case, from the respective weights of engine, tender, and load, according to the above rules. The friction of each engine was determined by the author separately, and I have used the invidual amount, so found, in preference to the mean of all, as the difference between them is considerable.

Experiment.	Total resistance. lbs.		Per sq. inch. lbs.	Friction of engine. lbs.
II.	1305.26	$\times \frac{5.887}{226}$	= 34	 152
III.	517.85	$\times \frac{5.887}{226}$	= 13.49	 152
IV.	429.20	$\times \frac{5.887}{226}$	= 11.17	 152
V.	485.35	$\times \frac{5.887}{194 \cdot 40}$	= 14.68	 187
VI.	487.63	$\times \frac{5.887}{190}$	= 15.10	 136
VII.	903.06	$\times \frac{5.887}{190}$	= 27.97	 108
VIII.	441.09	$\times \frac{5.887}{190}$	= 13.66	 108
IX.	548.20	$\times \frac{5.887}{190}$	= 16.97	 109
X.	614.34	$\times \frac{5.887}{190}$	= 19.03	 109

(*Treatise on Locomotives*, pages 134 to 156, and 169.)

4.185 cubic feet, the volume described by the pistons per double stroke, × 54.51 double strokes per minute = 228.12 cubic feet of steam consumed per minute; which, ÷ 0.517 cubic feet of water, evaporated per minute = 441 to 1, or ratio of the volumes of steam, and water. By referring to the table, it is found that 64 lbs. pressure per square inch, is the elastic force of steam ascribed to this ratio; deducting from which 14.71 lbs., we obtain 49.29 lbs. as the pressure per square inch, on the pistons, above the atmosphere.

Table X. contains the reduction of each experiment to terms of horses power, and exhibits under that denomination, 1st, the absolute power resulting from the steam used; 2dly, that required to overcome the assigned resistance; 3dly, their difference; 4th, the power which balances the gross, and useful duty. The consumption of water as steam, and of coke, per hour, and per horse power, under the above respective heads is also given; which, with the assistance of the remarks in the sequel, will enable us to compare the expenditure of power, with the effects produced according to the data of the different experimenters; and, finally, to test the accuracy of these data, by bringing the performance of the locomotive engines, for equal expenditure of power, into comparison with other engines in Table VI. The horse-powers have been computed as follows: example, Atlas, Experiment I.

For columns 33 and 34 the results are thus found; viz. for column 33;

$$\underset{\text{Pressure: lbs. per square inch.}}{49.29} \times \left(\frac{\underset{\text{Area of the pistons: inches.}}{226} \times \underset{\text{Length of double stroke: feet.}}{2.66} \times \underset{\text{No. of double strokes per min.}}{54.51}}{33000 \text{ lbs.}}\right) = \underset{\text{Horses power.}}{48.93}$$

and in like manner, for column 34.

For the gross duty (column 36);

$$\underset{\text{Effect in lbs. raised one foot.}}{257{,}813{,}952} \div \underset{\text{Minutes.}}{182}, \text{ and again by } \underset{\text{lbs.}}{33000} = \underset{\text{Horses power.}}{42.92}$$

and in like manner for the useful duty (column 37).

Column 38 shews the difference in horses-power between the whole power of the steam, and that required to overcome the resistances and frictions assigned, in Experiments I. to XII.; and this difference, according to the data of the experimenters, is the power consumed in blowing the fire.

The quantities of water and coke, in Table X., are the whole amounts actually expended by each engine, excepting when hereafter specially mentioned as corrected for ascertained waste: the term, "effective horse-power," column 35, will be explained in the sequel.

These three Tables—the columns of which are numbered forward, for more convenient reference, as if composing one continuous Table—thus present several modes of comparing the respective results of the experiments with each other, and with those of other classes of engine. For this purpose, the horse-power deduced from the gross duty will be preferable to the duty itself, as velocity is involved in the expression; and I propose, first, to apply the weight of water as steam expended in the production of that horse-power, as a measure of economical effect, and as a test of the accuracy of the data of resistance assigned by the different experimenters.

It is necessary to premise that the space passed over by the engines, in all the experiments, is presumed to be on a level plane. M. de Pambour * for Experiments I. to X., has adopted two measures of the distance travelled by his engines on the Liverpool and Manchester railway, in order to obtain a true comparison between them. When help was given by one or more engines in ascending inclines, he considers that help to have balanced the additional resistance of the acclivities, and the railway, consequently, to have been a dead level for the whole $29\frac{1}{2}$ miles traversed; and, when help was not given, he calculates the additional resistance, as equivalent to 5 miles on a level. In those cases, therefore, in which the train was unassisted, the space passed over is called $34\frac{1}{2}$ miles, and, when assisted, $29\frac{1}{2}$ miles. Thus, the velocities are calculated, in each experiment, from the distances assigned by the author, the time remaining unchanged, or that actually occupied by the trip. This is a rude method, but it answers the purpose for relative comparison, and it is the less necessary to shew its erroneousness, as it will hereafter appear that, in a later Work, M. de Pambour has selected for investigation, experiments on one of the engines in the Table, to which he has applied a new velocity, and revised data of resistance.

In column 11, the entire consumption of water is given, and from it the sums are derived in columns 39 and 41 to 45; the duty and the power are thus estimated by the whole weight of water as steam actually expended in each instance; but, the author informs us, (p. 184,) that we must deduct $\frac{1}{4}$th for waste, as only $\frac{3}{4}$ths of the water evaporated entered the cylinders, as steam. For relative comparison, therefore, the sums suffice, but they must, in each case, be diminished by one fourth in order to ascertain the effective performance of the steam, and to compare it with the engines in Table VI. The *effective*

* Treatise on Locomotives, pp. 323 to 325.

horse-power in the latter Table excludes, as does the *tractive*, or gross duty horse-power, (column 36,) all consideration of the friction of the engines unloaded, or loaded, and the comparison holds good, in both, for the expenditure of steam to produce equal effects, with the correction of $\frac{1}{4}$th for the locomotives.

Taking Experiments I. and II., (column 44,) we find that according to M. de Pambour, $47\frac{1}{2}$ and $50\frac{3}{4}$ lbs. of water as steam, only, have been consumed to generate a horse's-power, or to raise 33000 lbs. one foot per minute. The first result is perhaps within possibility at the pressure, but the second is impossible with a pressure of 29.79 lbs. per square inch, as, at that pressure, the expenditure of steam by the common non-condensing engine for a given power exceeds that of the rotative condensing engine, and we know the latter to consume 70 lbs. of water as steam for its maximum effect (Experiment V., Table VI.). It appears also, (column 31,) that a *vacuum* existed on the opposite side of the pistons in both these experiments; which is, also, an impossible result: we must, therefore, conclude either that the resistance of 8 lbs. per ton assigned to the load at 15 miles per hour is considerably too high, or that the evaporative data are exceedingly incorrect.

Experiment VIII., on the *Leeds* engine, satisfies the conditions of the non-condensing principle in the consumption of steam, at the pressure of 13.66 lbs., (as determined by Experiment III. in Table VI.,) inasmuch as, at the velocity of 26.70 miles per hour, the gross load being 44.08 tons, the horse-power required 123 lbs. of water as steam to create it. Thus, at that velocity, and with that load, 8 lbs. per ton was the sum of the entire resistance overcome by the steam, exclusive of the friction of the loaded engine. But the whole of this resistance must not be confounded with, nor considered as, tractive force; it includes the amount, whatever it might be, required to overcome the surplus pressure from the blast above the atmosphere, which must be ascertained, and deducted, before the true power, and the true consumption of water as steam, for tractive effort, can be determined. On this head the author was silent, and as before shewn, in explaining the Tables, I have endeavoured to discover the intensity of the blast-pressure, by ascertaining, and comparing the total pressure of the steam on the pistons, with that arising from the assumed resistances.

It appears, from the ratio of the volumes of steam and water consumed in this experiment, that the total pressure on the pistons was 25.62 lbs. per square inch, of which, the force required to overcome the sum of the resistances was 13.66 lbs., and their difference 11.96 lbs. per square inch; so that

this difference, or blast-pressure was equal, within $\frac{1}{8}$th, to the whole pressure required by the friction of the loaded engine, and the resistance of the engine, tender, and train: and this blast-pressure reduced to power (column 38) was 27.41 horses, whereas, the tractive force, from the datum of 8 lbs. per ton, (column 36,) amounted only to 25.11 horses-power.

A superficial examination would thus have indicated the resistance to traction of 8 lbs. per ton as correct, under the conditions of this experiment; but, such a conclusion would be most erroneous, for the water, as steam, has not only been employed to do the duty, but to blow the fire, which two operations have required $25.11 + 27.41 = 52.52$ horses power; and the water hourly expended as steam 3087.29 lbs. $\div$ 52.52 horses power $=$ 58.78 lbs. consumed per effective horse power, per hour, instead of 123 lbs. before found.

The enquirer will naturally ask whence proceeds the error, for some great error must exist, as it is not credible that, when the sum of the active resistances amounted only to 13.66 lbs. pressure per square inch on the pistons, the passive, or blast resistance should nearly equal it. The enigma is readily solved by comparing these results with Experiment VII. on the same engine; and the conclusion is inevitable that either the resistances, or the consumption of water, or both of these data, are very incorrectly estimated, for we find (column 31) a nearly equal blast-resistance resulting from the use of 39.19 and 25.62 lbs. absolute pressure on the pistons, respectively, in the two cases.

The *Vesta*, Experiment V., having the highest velocity in the Table, viz. 31.60 miles per hour, is represented to have had an absolute pressure of 15.30 lbs. per square inch on the pistons; the difference between that, and the pressure deduced from the resistance being only 0.62 lbs., which represents the blast-pressure.

The absolute pressure on the pistons corresponds within 0.30 lbs. with that in Experiment III., Table VI.; but the water consumed as steam, (column 44,) diminished by $\frac{1}{4}$th for waste, is only 101.55 lbs. for the effective load per horse-power per hour by the locomotive; whereas, the fixed non-condensing engine, without a blast, required 120 lbs. per horse power per hour.

The *Vulcan*, Experiment VI., has an absolute pressure of 20.04 lbs., being the same as the non-condensing engine, Experiment II., Table VI.; and its consumption of water as steam is only 99.86 lbs. for the same effect which required for its performance by the latter 125 lbs. The former, too, had a counter resistance against its piston from the blast equal to 5.94 lbs. per square inch, equal in power (column 38) to 11.38 horses, which, being so much load in

addition to the duty done, at 8 lbs. per ton tractive effort, brings the effective power of the steam in the *Vulcan* to 38.57 horses, and the consumption of water per horse power per hour to 70.40 lbs., or equal to the condensing engine—an impossible result at the pressure assigned.

The errors, therefore, are great and manifest. Had the consumption of water as steam been accurately given for Experiment VIII., the conclusion would have been indisputable, that 8 lbs. per ton was the whole resistance overcome by the steam besides the proper friction of the loaded engine; but this would not coincide with M. de Pambour's determination that 8 lbs. per ton was the tractive effort required of the engine; for, it includes the resistance opposed by the blast; so that, even at a velocity of 26.07 miles per hour, the resistance to progressive motion is overrated at 8 lbs. per ton. Thus, it is palpable that no reliance can be placed on the datum of water; and it is equally evident that the datum of resistance is very inaccurate; so that nothing can be gathered from this analysis, but that the author has signally failed both in his observations of the steam generated, and in his estimations of the force opposed to progressive motion.

Recurring to the experiment with the *Vulcan* at 26.90 miles per hour, I have already shewn the resistance to have been greatly exaggerated, the evaporation greatly underrated, or that we must be credulous enough to believe the horse-power to have been generated in the locomotive by 70.40 lbs. of water as steam per hour, using the same pressure which requires 125 lbs. in the fixed non-condensing engine to produce a like effect, 70 lbs. being the consumption of the condensing engine. Results equally inconsistent with the capabilities of the locomotive are perceptible in every one of these experiments.

I cannot imagine any conclusion to be safer, or more irrefragable, than that a condensing engine placed on wheels, with water of condensation transported for its supply, and made to drag a train of wagons, or coaches along a railway, would require precisely the same power, in water as steam, to produce a given effect, as if its foundations were based on a rock. No change has taken place in the engine by rendering it locomotive; it is intrinsically, and practically the same machine; and like effects would necessarily result from a like expenditure of power, whether the engine were at rest, or in motion: consequently, the effect produced, of whatever nature it may be, is measurable by the expenditure of power. By parity of reasoning, the non-condensing engine is one and the same machine, whether fixed, or locomotive; excepting

that the latter must consume more power than the former to do equal work, at like pressures, by the amount of additional resistance arising from the contraction of its eduction pipes, in order to produce a fierce blast of steam through the chimney. No change in mechanical structure has been made, but, it is reasonable to conclude that, from its shorter stroke, shorter connecting rod, and greater velocity of its piston, at high speeds, the locomotive would be subject to greater friction than a fixed engine; which would necessarily augment its consumption of power. The crank-shaft, too, through which the power is transferred, is liable to strains, and shocks, from which the same part of a fixed engine is free; hence, another disadvantage, and additional waste of power. Thus, the fixed non-condensing engine is the more economical of the two; but, if M. de Pambour's data are correct, we must abandon all preconceived opinions, and all belief in the accuracy of preascertained results on the non-condensing engine; we must reverse our engineering creed, and acknowledge the fixed non-condensing engine, with its simple atmospheric resistance, to be far inferior, in economy of steam, to the locomotive, with its plus atmospheric resistance.

It is seen that, according to the author, the *Atlas* in the two first experiments, consumed $47\frac{1}{2}$ and $50\frac{3}{4}$ lbs. of water as steam (waste deducted) per horse-power per hour, (column 44,) which gross amounts have still to be reduced for the power consumed in overcoming the blast pressure. We shall now see results more surprising than the foregoing, for it appears that though the resistance required 49.77 lbs. pressure per square inch on the pistons, in the one case, and 34 lbs. in the other, to overcome it, there existed no counter pressure in the blast pipe; but, on the contrary, a *vacuum*, (column 31,) amounting in the first experiment to nearly $\frac{1}{2}$ lb., and in the second, to $4\frac{1}{4}$ lbs. per square inch; column 38 shews, consequently, that less power was created by the steam, in these two experiments, than balanced the assigned resistances, amounting in the second case to no less than 6.45 horses-power. These are the results flowing from the comparison of the absolute pressure and power deduced from the ratios of the volumes of steam to water, with the power and pressure deduced from the respective sums of resistance assigned by the author. Hence, we are called upon to believe the authenticity of data which, in the case of the *Atlas*, Experiment II., establish a resistance equal to a pressure of 34 lbs. per square inch, with a *vacuum in the blast pipe;* and, in the case of the *Leeds*, Experiment VIII., a resistance equal to 13.66 lbs. on the piston, and *a counter pressure of* 11.96 *lbs. per square inch in the blast pipe;* we

are required to believe that, with an active pressure on the pistons $2\frac{1}{2}$ times greater in the one case than the other, the passive pressure against the pistons is $2\frac{1}{2}$ times less.

Let it not be thought that these cited cases are exceptions to the accuracy of others.

The *Vesta*, Experiment V., having a velocity of 31.60 miles per hour, exhibits a blast pressure of only $\frac{62}{100}$ths of a pound per square inch; whilst the *Vulcan*, Experiment VI., at 26.90 miles per hour, shews 5.94 lbs. per square inch as the counter resistance; and this last, whose velocity is the same as the *Leeds*, Experiment VIII., appears to have only half the amount of blast resistance to surmount, as the *Leeds*. Similar discrepances appear in all of them. It is however, preferable, and fairer to examine the authenticity of the data by results on the same engine. Thus, we should conclude, from Experiments I. and II. on the *Atlas*, that the resistance occasioned by the blast diminishes with the velocity, and with the active pressure; whereas, by comparing Experiments II. and III. on the same engine, a great increase in blast pressure is shewn at the higher velocity. In the *Leeds* the counter resistance is the same at two very different velocities, and two very different active pressures; in the *Fury* (the experiments on which engine will hereafter be shewn to be very anomalous) the blast pressure diminishes with the velocity of the piston, whilst the active pressure increases at the higher velocity. These discrepances arise out of extreme inaccuracy in the data of water, and resistance; and possibly, of velocity also.

A few more remarks on this subject are still requisite to render strict justice to the author. The previous comparisons between the intensity of pressure on the pistons, arising from the data of water, and resistance, result from a close adherence to his own instructions respecting the mean allowance for waste steam. This mean was deduced from special experiments on the quantity of steam which escaped during the trips, as determined by the degree of the rising of the safety valves, in each instance; and, though I attach no faith to the exactness of the approximations to waste obtained by this method, and adopted by the author, still it may be urged that each experiment should be investigated, after corrections suited to its own particular case.

The rising of the valve*, Experiment I. on the *Atlas*, indicated a loss of $\frac{1}{6}$th of the steam, so that, to arrive at perfect correctness, we must deduct that

* Treatise on Locomotives, p. 75.

quantity instead of $\frac{1}{4}$th. Using, therefore, this correction for water consumed, and afterwards $\frac{1}{20}$th for waste steam, the ratio of steam to water is 390 to 1, and the pressure on the pistons 58.10 lbs. per square inch, which, by comparison with the pressure due to the ascertained sum of the resistances, would give 8.33 lbs. per square inch for the unascertained amount, or blast pressure. This result would seem to indicate an approach to the region of truth and credibility; but, unfortunately, a reference to column 32 destroys the hope of so easily extricating ourselves from this labyrinth of error, by exhibiting the fact that the pressure in the boiler (about which I should think there could be no mistake) was only 53.7 lbs., so that the pressure on the pistons could not have exceeded it, or have attained 58.10 lbs.! Granting, however, for an instant, that the pressure in the boiler might have been erroneously observed, and that the data for the resistances and water were, possibly, correct, let us see how the latter accord with the results of other engines. The gross duty or tractive power (column 36) is 42.92 horses, and the power required to overcome the blast pressure of 8.33 lbs., now found, would amount to 8.26 horses; so that the effective power of the steam was 51.18 horses. The water consumed per hour was 2703.07 lbs. − 453.84 lbs., for $\frac{1}{6}$th wasted, = 2269.23 lbs. which ÷ 51.18 horses = 44.33 lbs. of water as steam consumed per horse-power, per hour; so that the *Atlas*, in this experiment, did equal work with the non-condensing engines in Table VI., with little more than one third, and equal work with the rotative condensing-engines with little more than two thirds the respective expenditure of water, as steam, by those engines.

I will take one more example from the *Atlas*, viz., Experiment II., (the 2d, also, in the author's Treatise before referred to, p. 75). The rising of the valve indicated an actual loss of $\frac{1}{40}$th only; deducting, therefore, this quantity from the water, instead of $\frac{1}{4}$th, and afterwards $\frac{1}{20}$th, the ratio of the volumes of steam and water comes out as 472 to 1, and shews a pressure of 44.04 lbs. per square inch on the pistons, from which subtracting 34 lbs. required for resistance, 10.04 lbs. would be left for blast pressure. Now, the gross duty, or tractive power is (column 36) 44.65 horses, and the blast pressure is equivalent to 15.37 horses-power, making 60.02 effective horses-power. The whole water consumed per hour was 3018.81 lbs. which, diminished by $\frac{1}{40}$th = 2943.34 lbs. ÷ 60.02 = 49.03 lbs. as the consumption of water as steam per effective horse-power, per hour; which is, also, an impossible result.

I have thus applied the author's most minute corrections to his experiments; I have "traced the use of every pound of steam, in the effect produced," according to his own data; and the result is, that if those data be correct, the sooner we substitute non-condensing, for condensing engines, for all purposes, the better; for, it seems that the locomotive has a faculty of producing an equal effect, with only two-thirds of the expenditure of power required by the condensing engine.

The foregoing investigation of M. de Pambour's experiments was completed previous to the appearance of his "*New Theory of the Steam Engine*," but I have since computed all the pressures resulting from the ratio of the volumes of steam and water, by the elaborate and useful Table contained in this last work, which I have transferred to the end of these pages as necessary for reference. These pressures were originally deduced from a less ample, and, probably, less accurate table in the same author's "*Treatise on Locomotives*," page 204.

M. de Pambour has seen reason, since the first publication of his experiments, to modify his original data of resistance, by assigning relative quantities due to friction, and to the air, at all velocities. He has, also, supplied his original omission of blast pressure, by assigning a value for it, at all velocities. He has, thereby, enabled me to examine his revised opinions as contained in his "*New Theory of the Steam Engine*"; and it will presently be seen whether, and in what degree, he has corrected the errors pointed out in the experiments themselves, or in his deductions from them. The new, or amended data adopted by the author are;

1st. That, "when the velocity is 10 miles per hour for the engine, or 150 feet per minute for the piston, the pressure arising from the blast-pipe is 1.75 lbs. per square inch on the surface of the piston, and that it varies in the direct ratio of the velocity of the motion." (page 161).

2d. That, "the resistance of the air against a train of mean surface is 33 lbs. at the velocity of 10 miles per hour for the engine;" and "that it increases as the square of the velocity." (page 162).

3d. That, "the resistance to progressive motion, independent of the air, is 7 lbs. per ton." (page 35).

Using these data, the author has adduced two of his previous experiments on the *Leeds* engine, to shew the superior accuracy of his *New Theory*, over the theories of other writers on the subject. With these I have nothing to do, dealing, as my object professedly is, only with facts, or with data

assumed as facts, which I shall proceed to quote in the author's own words, referring to the number of the experiment corresponding with them in Tables VIII., IX., X. The following particulars are extracted from pages 34 to 41, "*New Theory of the Steam Engine.*"

Experiment XIII. "The locomotive *Leeds,* which has two cylinders of 11 inches diameter; stroke of the piston, 16 inches; wheels, 5 feet; weight, 7.07 tons; drew a load of 88.34 tons, ascending a plane inclined $\frac{1}{1300}$, at a velocity of 20.34 miles per hour, the effective pressure in the boiler being 54 lbs. per square inch, or the total pressure 68.71 lbs. per square inch."

Experiment XIV. "The same day, the same engine drew a load of 38.52 tons, descending a plane inclined $\frac{1}{1094}$, at the velocity of 29.09 miles per hour; the pressure in the boiler being precisely the same as in the preceding experiment, and the regulator opened to the same degree. These experiments are given, pp. 233, 234, of our *Treatise on Locomotives.* (1st Edition.)"

"Now, during the journey, of which the first of these experiments was a part, the engine evaporated 60.52 cubic feet of water per hour; (*Treatise on Locomotives,* 1st edition, p. 175;) which, after a deduction of $\frac{1}{5}$th for the loss of steam by the safety valve, measured as explained in Section VII., and of $\frac{1}{20}$th on the rest for the filling up of the vacant spaces of the cylinder, leaves an *effective* evaporation of .77 cubic feet of water per minute." The same evaporation is applied by the author to the 2d experiment.

He then proceeds to give "a detailed calculation of the effects produced" in the two cases, which I shall follow verbatim, separating only the friction and resistances of the engine, and tender, from that of the train, with the view of ultimately exhibiting the power usefully, and uselessly expended by a locomotive. In this work the author refers for an explanation of his present use of 7 lbs. per ton as the tractive resistance, and of his method of ascertaining the blast pressure, and air resistance, at different velocities, to the "2d edition of his *Treatise on Locomotives;*" but it is remarkable that this 2d edition, quoted by him as a necessary reference, has not yet seen the light; so that I am unable to elucidate these points.

DETAILED CALCULATION OF THE EFFECTS.

1st Case. (Experiment XIII.)

	lbs.	lbs.	lbs.
Resistance of the train (20 wagons) 83.34 tons, at 7 lbs. per ton		583	
Gravity of 83.34 tons, on plane rising $\frac{1}{1300}$		+143	
			726
Resistance of the air against the train at the velocity of the motion			134
Friction of the engine, without load	82		
Additional friction for a resistance equivalent to $\frac{916}{7} = 131$ tons	131		
		213	
Resistance of the tender 5 tons, at 7 lbs. per ton		35	
Gravity of engine and tender 12.07 tons on the plane		+ 21	
			269
For pressure caused by the blast-pipe, being at the velocity 3.4 lbs. per square inch on the piston			109
Total resistance overcome at the velocity of the wheel			1238

$$\therefore 1238 \times 5.9 \div 190.06 = \overset{\text{lbs. per sq. in. on piston}}{38.43}. \text{ and } 38.43 \times \left(\frac{190.06 \times 2.66 \times 114}{33000}\right) = \overset{\text{horses power.}}{67.11}.$$

2d Case. (Experiment XIV.)

	lbs.	lbs.	lbs.
Resistance of the train (7 wagons) 33.52 tons, at 7 lbs. per ton		234	
Gravity on plane descending $\frac{1}{1094}$		− 68	
			166
Resistance of the air against the train, at the velocity of the motion			282
Friction of the engine, without load	82		
Additional friction for a resistance equivalent to $\frac{459}{7} = 66$ tons	66		
		148	
Resistance of the tender, 5 tons, at 7 lbs. per ton		35	
		183	
Gravity of engine and tender 12.07 tons, on the plane		− 24	
			159
For pressure caused by the blast-pipe, being at the velocity 5.1 lbs. per square inch on the piston			164
Total resistance overcome at the velocity of the wheel			771

$$\therefore 771 \times 5.9 \div 190.06 = \overset{\text{lbs. per sq. in. on piston.}}{23.93}. \text{ and } 23.93 \times \left(\frac{190.06 \times 2.66 \times 162.41}{33000}\right) = \overset{\text{horses power.}}{59.50}$$

These data are elaborated in the Tables to the same terms as the other experiments, but with certain changes of value arising out of the assigned aliquot parts of the total resistance, which require notice; and this is the more necessary, because the accuracy of the author as an experimenter, together with the practical value of his experiments and deductions, are to be judged of by these new data, rather than by the foregoing investigation of his old data; inasmuch as they must be considered to be his conclusions matured by four years of reflection, and additional experience. By the subdivision of the frictions and resistances, *duty* can be properly estimated—as, also, *effective horse-power*—so as to bring these definitions of work and power, to a truer comparison with the same definition applied to other engines. I must, therefore, shew the manner of separating, and ascertaining these values, as respectively set down in the columns.

I consider the *duty* of a locomotive to be that appreciable effect which is performed by the engine independent of all opposition from the air to the progressive motion of a train. It is only in this view that duty strictly corresponds with its received definition; for the practical performance of a pumping engine estimated by the duty done, or weight of water raised one foot, by a pound of steam, omits taking into account the friction of the water in the pipes; the duty is taken on the weight raised, only. The weight raised by a locomotive, in the same sense of duty, is that which is necessary to communicate motion to the train; and this weight, or tractive force, is now assumed by the author to be equal to 7 lbs. per ton of matter moved on a level plane, and to be a constant quantity, at all velocities. In the cases cited, he increases, or diminishes this weight, in ascending or descending inclines, on the principle that the tractive force "is the gravity along the plane, and equal to the mass that is to be moved, divided by the number that marks the inclination of the plane." (*Treatise on Locomotives*, p. 206).

The true *effective power* of the steam is here considered to be that expended in overcoming every species of resistance, excepting the friction of the engine itself, and the counter pressure of the blast. It is not in every case that this amount can be arrived at, as previously shewn, (page 51 of this paper,) but according to the data before us it is determinable. It is the gross duty just explained, plus the resistance of the air. The proportionate values of these several resistances are thus found:

1st Case. (Experiment XIII.)

	lbs.		lbs. per ton on the gross load.
Resistance of train and tender 88.34 Tons, at 7 lbs. per ton	618		
Gravity of 95.41 tons (engine, tender, and train) on plane rising $\frac{1}{1300}$	+164		
Tractive force for computing the gross duty — At the velocity of the wheel	782	=	8.19
Resistance of the air, at 20.34 miles per hour	134	=	1.41
Tractive force for computing effective power. — At the velocity of the wheel	916	=	9.60
Friction of loaded engine	213	=	2.23
Resistance from blast-pipe	109	=	1.14
Total resistance at the velocity of the wheel	1238	=	12.97

2d Case. (Experiment XIV.)

	lbs.		lbs. per ton on the gross load.
Resistance of train and tender 38.52 Tons, at 7 lbs. per ton	270		
Gravity of 45.59 tons descending plane $\frac{1}{1094}$	−93		
Tractive force for computing gross duty. — At the velocity of the wheel	177	=	3.88
Resistance of the air at 29·09 miles per hour	282	=	6.72
Tractive force for computing effective power. — At the velocity of the wheel	459	=	10.60
Friction of loaded engine	148	=	3.24
Resistance from blast-pipe	164	=	3.07
Total resistance at the velocity of the wheel	771		16.91

Supposing these data to be correct, the decomposition of the component parts of the total resistance enables us to determine the amount of steam power consumed in overcoming each item of resistance, at the two velocities; and I have, accordingly, worked out the particulars as follows:

	Exp. XIII. Velocity 20.34 miles per ho. H. P.	Exp. XIV. Velocity 29.09 miles per ho. H. P.	
Resistance of the train	39.355	12.810	Useful effect.
Ditto of the air	7.263	21.762	
Ditto of engine and tender, and friction of the engine.	14.582	12.270	
Ditto caused by the blast	5.908	12.656	
Absolute power of the steam	67.108	59.498	
Effective ditto	49.654	35.422	
Ineffective ditto	17.454	24.076	
Useful power of the engine	39.355	12.810	
Useless ditto	27.753	46.688	
	67.108	59.498	

The *useful effect* is the useful duty arising from the resistance to the train only, and it thus appears from these two experiments (always premising the data to be correct) that at a velocity of 29.09 miles per hour, it required more than three times as much power to realize the same useful effect, as at 20.34 miles per hour; a result, of which we shall, hereafter, see great reason to doubt the truth.

In order to establish a true comparison between these new results and the engines in Table VI., column 16, the whole water must be divided by the sum of the effective power, plus the additional power required to balance the blast resistance; these amount for the 1st case to $55\frac{1}{2}$, and for the 2d case to 48 horses, which gives the water consumed respectively per horse, per hour, as $54\frac{1}{2}$ and 63 lbs. In all the columns the water has been diminished by $\frac{1}{5}$th for waste according to the author's instructions. Thus, it is evident that the original error of far too high an estimate of resistance still clings to the data as amended, for it is utterly impossible that the locomotive should accomplish an equal effect, with $\frac{1}{5}$th less steam than the condensing-engine. To go over this ground again, would be a mere repetition of arguments previously used.

It is also demonstrable that, on the supposition of the data being correct for the first, they are incorrect for the second experiment; for the author has left nothing to be guessed at; no unknown quantities have to be found from known ones; he has assigned the particular values of every component portion, as well as the sum total of the resistances to be overcome. He has, also, informed us that in the two experiments " the pressure in the boiler was precisely the same, and the regulator opened to the same degree." The power

applied in the two cases was, consequently, precisely equal, and equal weights of water as steam passed through the cylinders in equal times; whence, it results that the effects should have been similar. The expenditure of power was, however, greater by more than a third in the second than in the first case, to produce like effects, for we see that the effective horse-power required 85.43 lbs. of water as steam in the second, and only 60.94 lbs. in the first. Either, then, the sum of the resistances overcome must have been greater by one third, than the author's estimate, in the 2d experiment, or it was too small in the 1st, or he has erred in applying the same measure of water to both.

It is very remarkable that such an enormous discrepancy should have escaped M. de Pambour's notice. In his *Treatise on Locomotives*, pp. 310 to 312, he states a near parallel to these two experiments by supposing a case of "the same engine, with the same pressure in the boiler, travelling the same distance with two different loads. The distance travelled being the same, the number of turns of the wheel, and consequently of strokes of the piston, or cylinders of steam expended will be the same in the two cases;"—he proceeds, "the mass, or weight of steam expended, will be in each case in the ratio of the pressure in the cylinder. The weight of the steam being equal to that of the water that generated it, the weights of water evaporated will then be to each other as the pressures in the cylinder, or, in other words, as the resistances on the piston." In the "*New Theory of the Steam Engine*," the author has dwelt at great length, and occupied many pages, to prove this indisputable, and, so far as I know, never disputed fact; viz. that the steam, in every engine, necessarily assumes in the cylinder, the pressure due to the resistance it has to overcome; or, to use his own phrase, that the pressure of the steam, and the resistance "*equilibrate*." Now, the author has given us the resistances on the piston, which amount in the 1st case to 38.43 lbs., and in the 2d, to 23.93 lbs. per square inch; and yet he assumes an equal expenditure of water as steam, in equal times, in the two cases, and applies it (Chap. I. pp. 43, 44, *New Theory of the Steam Engine*) to demonstrate the truth of his own theory, and the inaccuracy of other theories. To be consistent, however, with his own rule, above quoted, viz. that "the weights of water consumed as steam are to each other as the resistances against the piston," it is obvious that if, in the 1st case, 3026 lbs. of steam passed through the cylinders in an hour, 2166 lbs. only would have been expended in the 2d case.

The expenditure of water as steam would have been equal, in equal times,

or alike in both these cases, had the resistances been alike; but it is evident that the formulæ applied to the estimation of the latter, are insufficient for the solution of the problem, for if, as asserted, "the pressure in the boiler were precisely the same, and the regulator open to the same degree," in the second, as in the first experiment, equal power must have been generated and expended in the same time; though, at the higher velocity, the lighter load was moved through a greater space in that time.

Had M. de Pambour reduced his data to the terms of value in these Tables, he must inevitably have discovered the numerous errors of fact, and deduction, which are now brought to light; had he worked out the arithmetical computations of which his formulæ are merely symbolical, and compared their results with the performance of the Albion Mills engine alone (which he has analysed in his "*New Theory*"), he would have seen reason to doubt the accuracy of his experiments.

I have already commented upon the nice and numerous precautions requisite to give accuracy to the method of finding the pressure on the piston from the ratio of the volume of steam consumed to that of the water which produced it. I am disposed to believe this test to be as perfect as it is beautiful; I entirely agree with the author in his estimation of its practical value; but I will now adduce a most remarkable instance of its coincidence with error of resistance, as well as an instance of its utility in detecting error.

By referring to columns 29 and 30, Experiment XIII.,—the first of the two cases under review—it is seen that the pressures on the piston deduced by the method of the volume, and from the sum of the assigned resistances are identical; yet we know one or both of these data to be incorrect, and greatly so, since an equal power is exhibited as having been produced by $\frac{1}{5}$th smaller consumption of steam than the best specimen of the condensing engine, and by only one half the necessary e penditure of the fixed non-condensing engine. Had we, therefore, no other means of ascertaining the accuracy of an assigned expenditure of power than that afforded by the comparison of the volumes of steam and water, the volume test would, in this case, have confirmed a great error in the datum of resistance, for the errors of water, and the errors of resistance coincide.

By referring to the same columns, Experiment XIV., we find the pressure per volume insufficient to balance the resistance, and that the whole power, indicated by the volume test, falls short of the required amount, by nearly 6

horses power. Supposing the resistances assigned to have been correct, the ratio of the volume of steam to that of the water consumed, should, in this case have been 700, instead of 741 to 1, and .81 cubic foot of water must have passed through the cylinders in the shape of steam, per minute, instead of .77 cubic foot, to have impressed upon the pistons a force of 23.93 lbs. per square inch, which is the pressure deduced from the resistance.—But, we have already seen that, if the quantity of water were correctly taken in the 1st case a less quantity must have been consumed in the 2d, as the load upon the pistons of the engines in the two experiments deduced from their velocity and assigned resistances, differed in the ratio of 38.43 to 23.93; and the water as steam consumed in equal times must necessarily have varied in the same ratio, or as 3026 lbs. to 2166 lbs. The pressure, therefore, deduced from the corrected datum of 2166 lbs. for the 2d case, after deducting $\frac{1}{20}$th for waste, should accurately balance the resistance; but dividing 2063 lbs. (the sum last found) by 60 minutes, the duration of both experiments, 0.55 cubic foot is the consumption of water per minute, and 1037 the ratio of the volumes of steam and water; whence, from the author's Table, the pressure per square inch on the pistons would be $24.81 - 14.71 = 10.10$ lbs. only; whereas, the resistance required 23.93 lbs. pressure per square inch to balance it.

The author might object to the comparison of steam consumed by the effective power, and consider the proof of accuracy to be more justly dependent on the relative consumption of steam by the absolute power of each engine.

The absolute power in the two cases, is as 67.24 to 53.55 (column 33,) and the expenditure of water as steam by this ratio should have been as 3026 to 2409 lbs.; and the latter corrected for waste, would give 935 to 1 as the relative volumes of steam and water $= 28.20 - 14.71 = 13.49$ lbs. per square inch on the pistons, whilst the resistance was 23.93 lbs.!

It would be fruitless to pursue this analysis further, and vain to attempt the rectification of errors—a task which properly belongs to the author. It is manifest that, in both these cases, as well as in the former series, resistances have been assigned which the force of the steam employed was insufficient to overcome; it is manifest that the sum of the resistances at the different velocities—whether arising from erroneous estimations of the friction of motion, friction of the engine loaded, and unloaded, resistance of the air, or pressure occasioned by the blast—is purely hypothetical, and has no foundation in fact; and, it is manifest that the evaporative data are extremely fallacious.

To arrive at synthetical results was the chief object of M. de Pambour's analytical labours; for, the intrinsic value of his labours depends on the accordance established between the theoretical and practical performance of the locomotive engine at various velocities, and with various loads: it is, therefore, not a little singular that this ingenious and truth-seeking experimenter should have omitted to bring the whole series of his experiments into juxta-position, and have compared his estimation of resistance with the expenditure of steam, as in Tables VIII., IX., X. He would then have discovered the little reliance to be placed on quantities deduced from mere theory, and have perceived the necessity of obtaining evaporative data from much more careful, and satisfactory observations.

Experiments by Mr. Robert Stephenson. The particulars of Experiments XI. and XII. are extracted from a separate publication of Mr. Robert Stephenson's elaborate article on the locomotive engine, contributed by him to Tredgold's work on the Steam Engine, second edition*.

* "The power of a locomotive engine cannot readily be estimated in the same manner as that of other engines, by taking the actual force upon the piston, and the velocity of its motion; for it is very difficult to ascertain the effective pressure of the steam upon the piston, in consequence of its differing often very considerably from that of the steam in the boiler, and because of the large amount of the resistance of the waste steam, owing to the great velocity with which the piston moves. The power is also different at different velocities, as these circumstances vary with the velocity. The only correct means, therefore, of ascertaining the power of a locomotive, is by deducing it from the work which it is capable of performing.

"This engine has drawn a load up an inclined plane that was equivalent to 220 tons gross weight upon a level, (including engine and tender,) at a velocity of 14 miles an hour; which appeared to be about the extent of the power of the engine with the steam at the usual pressure of 50 lbs. on the square inch, in the boiler. The force required to perform this, is about 2,050 lbs. moving at that velocity; which is equal to 77 horse-power. The effective pressure on the piston, or the actual force with which it was propelled, must therefore have been $47\frac{1}{2}$ lbs. per square inch, instead of 50 lbs., which was the pressure of the steam in the boiler; the difference being the power that was lost by the resistance of the waste steam, and by the diminution of the pressure of the steam, in consequence of the throttling or wire drawing that takes place in passing through the ports of the cylinders, and which was in this instance very inconsiderable.

"The horse-power of an engine is less when drawing a lighter load at a greater velocity, as the loss of power from the throttling and the waste steam is then increased; and it would cease altogether at a certain speed, varying according to the proportions of the engine, when the velocity of the piston became as great as that with which the steam can enter into the cylinder, or the waste steam escape. This engine has drawn 40 tons at 35 miles an hour, which is equivalent to 40 horse-power; in which case the effective pressure on the piston must have been only 10 lbs. on the square inch, or but one-fifth of that of the steam in the boiler. This shews the great loss

The author has adopted M. de Pambour's original datum of resistance, viz. 8 lbs. per ton, for calculating the power of this engine at the two velocities cited. The fallacy of that estimate having already been demonstrated, it is unnecessary to submit these results to the same minute investigation, as in the previous instances; yet a few remarks are requisite to point out some remarkable discrepances between the power represented to be employed, and its estimated effect, in the two experiments.

It cannot be said that Mr. Stephenson has not reduced the data of resistance and velocity to terms of power, for he has discovered and stated that this locomotive, " when working with a full load, consumes a cubic foot of water as steam per hour for each horse-power," which, he observes, " is also the usual

of power in working the engine so quickly; the loss at 14 miles an hour having been very little. The loss of power is lessened by making the wheels larger, the velocity of the piston being by that means diminished; and they are consequently made as large as is practicable.

" The power of a locomotive engine is limited only by the evaporating power of the boiler, or the number of cylinders full of steam that can be supplied in a given time, by which the velocity of the piston is determined; while in other steam engines the size of the cylinder is the limit to the power, as a sufficient quantity of steam to supply them can be readily obtained by increasing the size or number of the boilers, which cannot be done in a locomotive. This engine is capable of evaporating 77 cubic feet of water per hour, or eight gallons in a minute; and the large amount of this power causes its great superiority to the old locomotives, which could evaporate only about 16 cubic feet per hour.

" The consumption of fuel per mile for every ton of the gross load is about a quarter of a pound, and that of the water is rather less than a quarter of a gallon; the consumption increasing to nearly one half, according as the engine is less fully loaded, being proportionally greater with a light load; the consumption of water when working with a full load, is a cubic foot per hour for each horse-power, which is also the usual proportion in stationary engines although they condense the steam. About 8 lbs. of fuel is required to evaporate a cubic foot of water, being nearly the same as in stationary engines; but in the old locomotives as much as 18 lbs. was required, in consequence of their having so small a heating surface, which was only about two and a half square feet for each foot of water evaporated per hour; the proportion in the present one being five and a half square feet, and in stationary engines as much as eight.

" The great perfection of the present locomotives, and their superiority to the old ones, is caused not so much by the application of new inventions to them, as by the combination of many former ones, and the uniting together several plans which, separately, would be but of small value. Their great power and velocity, for example, could not have been obtained without the rapid means of generating steam afforded by the use of the tubes; and the tubes would have been useless, without the powerful draught produced by the blast, which increases in intensity with the velocity, and with the necessity for its increased action."—(*Description of the Patent Locomotive Steam Engine, by Messrs. Robert Stephenson and Co.*, pages 66 and 67.)

proportion in stationary engines, although they condense their steam." He has also, advanced another still more extraordinary proposition, viz. that, with a lighter load, the same engine consumes nearly half as much more water as steam to produce an equal mechanical effect, as when fully loaded. These are paradoxes naturally resulting from the adoption of M. de Pambour's too high estimate of tractive force, and from considering the tractive force, *per ton* of matter moved, to be a constant quantity at all velocities. It is certainly singular that, with so clear a perception of the conclusions necessarily flowing from these data, and exhibited by his own computations, Mr. Stephenson should not have hesitated in adopting them; for, even if the locomotive were a condensing engine, it is evident that its effective power must undergo diminution by the amount requisite to move itself and its tender, a force which, even at the lowest velocity, is far from being inconsiderable. It is equally evident that a stationary non-condensing engine consumes more steam, for equal effect, than a stationary condensing engine; and that the locomotive, compared with the former, has not only to expend steam to move itself, and its tender, but, also, to overcome the resistance caused by the blast.

The author has given an excellent commentary on this obvious fact, for he states, in the third paragraph quoted, that "the horse-power of the engine would cease altogether at a certain speed." The meaning of that sentence is not very obvious, but I imagine it to be the author's intention to convey to his readers, that there is a velocity at which the whole power of the steam would be absorbed in moving the locomotive itself, and its tender; consequently, that the engine could drag no load at that velocity, with the identical quantity and pressure of steam then applied. There is, doubtless, a velocity at which the useful power of the *engine*, commercially speaking, ceases; nevertheless, the effective power of the *steam* is the same at all velocities of the engine, under all loads, for an equal consumption of water as steam; and, it will hereafter be shewn that an equal mechanical effect results, at all velocities, from an equal expenditure of power in the locomotive, as in all other engines.

In the fourth paragraph the author observes that "the power of a locomotive engine is limited only by the evaporative power of the boiler," &c. This definition is clearly imperfect, for no power of steam could impart progressive motion to the engine, if loaded beyond the force of its adhesion to the rails; that force is, therefore, the dynamic limit; the supply of steam is a mere practical condition of the boiler.

These two experiments supply additional means for disproving the accuracy of M. de Pambour's data; they likewise furnish powerful incentives to experimenters to be vigilant in obtaining precise observations of evaporative facts, for they contain within themselves, abundant proofs of error in the quantities assigned to the consumption of water as steam.

Experiment XI. The sum of the resistances distributed over the pistons (column 29) is 47.54 lbs. per square inch, being the same as calculated by Mr. Stephenson from M. de Pambour's data; the pressure in the boiler is stated at 50 lbs., "the difference (or $2\frac{1}{2}$ lbs.) being," according to the author, "the power that was lost by the resistance of the waste steam, &c., &c.—which was in this case very inconsiderable." This method of finding the blast pressure does not suffice, for the pressure in the boiler is totally irrespective of the force on the pistons; the resistance caused by the blast is the difference between the absolute pressure of the steam in the cylinders, and that required to balance the sum of the ascertained resistances. The blast resistance is the unascertained portion of the total resistance opposed to the steam, and is discoverable when the absolute force exerted by the steam on the pistons is known. Now, if the evaporative data are correct, it would appear by the ratio which the volume of steam consumed bears to that of the water which produced it ($\frac{1}{20}$th being deducted for waste) that the absolute pressure upon the pistons in this case amounted to 81.95 lbs. per square inch; but there was only 50 lbs. in the boiler! If, therefore, 77 cubic feet of water passed through the cylinders in an hour, in the shape of *pure* steam, the blast pressure, or counter effort above the atmosphere, was 34.41 lbs., instead of $2\frac{1}{2}$ lbs., per square inch on the pistons.

The author has given no instructions for diminishing the consumption of water as steam, for any issue through the safety valve, priming, or other waste; and if none of the steam were wasted, it must have exerted the above force on the pistons; but, perceiving an error, I applied M. de Pambour's correction of $\frac{1}{4}$th in computing the pressure per volume, (column 28,) and even with this allowance, it is seen that the absolute pressure amounted to more than $5\frac{1}{2}$ lbs. above that in the boiler, which is also impossible, if the boiler pressure be correctly stated. In order to make the pressure on the pistons coincide with that in the boiler, (to which, however, strictly speaking, it can never attain, in a locomotive,) $\frac{2}{7}$ths of the whole water said to be evaporated must be subtracted for loss, and the horse-power would then be performed, according to the datum of resistance, by 44.8 lbs. instead of by a cubic foot, per hour; and, then, there

is nothing left for blast pressure, with a presumed force of $47\frac{1}{2}$ lbs. per square inch in the cylinder.

Mr. Stephenson assumes, with M. de Pambour, that the resistance from the blast increases as the pressure on the piston diminishes. The probability of the correctness of this assumption will be hereafter examined. The volume test shews either that 77 cubic feet of water did not pass through the cylinders as steam; or that, if such were the case, a pressure of above 80 lbs. must have existed in the boiler; and, if so, the blast resistance must have exceeded 34 lbs. per square inch. The truth probably is that the boiler *primed* to a great and unobserved extent, and thus vitiated the experiment, both as regards evaporation, and the expenditure of power.

Experiment XII. In this case I have assumed an equal evaporation in the same time, as in the foregoing experiment; and if $\frac{2}{7}$ths were deducted for waste, the blast pressure would be less than nothing—or a vacuum; for, with the subtraction of $\frac{1}{4}$th for waste, as in the Table, the absolute pressure amounts only to 11.10 lbs., whilst the resistance required 10 lbs. per square inch: and if, contrary to demonstration, it be considered possible that the 77 cubic feet of water were converted into pure steam, and that this quantity passed through the cylinders in the hour, the blast pressure would equal the whole force required to balance the assigned resistance; for the absolute pressure on the pistons would have amounted to 20.70 lbs. per square inch, whilst the sum of the ascertained resistances was only 10 lbs.

Making an equal deduction of one fourth for waste of water, in the two cases, and $\frac{1}{20}$th for ineffective steam in the passages, the blast pressure would have been (as set down in column 30) 8.15 lbs. in Experiment XI., and 1.10 lbs. in Experiment XII., thus exhibiting, with an assumed equal consumption of water as steam, in equal times, the very reverse of the doctrine that the blast pressure is in the inverse ratio of the absolute pressure on the piston—or, in M. de Pambour's words, that it increases with the velocity of the piston. The data of resistance and evaporation are, however, proved to be so erroneous, that these experiments present no facts on which to ground either a measure of the force of steam actually expended, or a measure of the tractive effort actually exerted; consequently, no truthful approximation can be derived from them as to the value of blast pressure.

It is proper to notice that in Experiment XII., I have assumed the same consumption of water as in the first case, being unable to reconcile the author's

statement of evaporation with his deductions as to the quantity of water consumed per ton of gross load per mile. In the fifth paragraph quoted, he applies a cubic foot per hour per horse-power to the first experiment, and that power, from his data of resistance, being 77 horses, there is an accordance with the assigned evaporation of 77 cubic feet per hour. But, in the same sentence, he says that the consumption of water per ton per mile is something less than a quarter of a gallon, which on the load of 220 tons for 14 miles amounts to nearly 7,700 lbs. instead of 77 cubic feet or 4,812 lbs. as before stated; and 220 tons was very nearly the maximum load of that engine. In the second case the expenditure of water at nearly half a gallon per ton per mile, would amount to nearly 7,000 lbs. I have assumed 4,812 lbs., and shewn that consumption to have greatly exceeded possibility.

Experiments by Mr. Nicholas Wood.

Mr. Wood's series of experiments with seven engines on the Great Western, and two engines on the London and Birmingham Railway, will be found of great use in illustrating another section of this subject; but though they embrace the question of expenditure of power, Mr. Wood has thrown no new light on the resistance opposed to progressive motion. He has, indeed, in his "Report to the Directors of the Great Western Railway," adopted Dr. Lardner's conclusions relative to the amount of resistance arising from friction, as well as from the atmosphere; but as Dr. Lardner has made additional experiments on these values, and has since furnished the means of testing their accuracy by a practical experiment on the Grand Junction Railway, it is needless to reduce Mr. Wood's to the terms of the Tables; I shall, therefore, pass to the consideration of the latter.

Experiments by Dr. Lardner.

The 15 experiments recorded in the following Table were communicated by Dr. Lardner to the British Association for the Advancement of Science in August last. Their purport was to determine the resistance opposed to progressive motion on railways. The experiments consisted in dismissing trains at various speeds from the summit of inclined planes, and in observing their velocity when it became uniform; the resistance, at such velocity, being inferred to be equal to the force representing the gravity of the mass along the plane. Table XI. contains the particulars of the experiments together with the sum of resistance, or tractive force, computed from the author's data, and placed in the last six columns. The resistance is likewise separated into two amounts, the one supposed to balance atmospheric opposition, the other supposed to be the value of the friction of the axles, and the friction between the wheels of the

carriages and rails. These component portions of the total resistance are also reduced, in each experiment, to their proportionate values per ton of matter moved. I have been obliged to assume an amount for friction as the author fixed no positive quantity for it, but he considers it to be nearly a *constant* at all velocities, and though 6 lbs. per ton may be somewhat higher than his standard, as collected from his remarks at Birmingham, and his Report to the British Association, in Vol. VII., the subsequent investigation will shew that the separation of the sum of the resistance into quantities attributable to this or that source, is of no moment as regards the objects of this Paper.

Table XI.

Experiments.	Weight.	Number of coaches.	State of the wind.	Inclination of plane.	Uniform velocity acquired.	Resistance or tractive force. Total.	Per ton.	From the air.	From friction.	From the air per ton.	From friction per ton.
	Tons.			One in	Miles per Hour.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1	15.6	4	Favorable.	96	31.20	364	23.33	270.4	93.6	17.33	6
2	18.0	...	...	...	33.72	420	23.33	312.0	108.0	17.33	6
3	18.0	...	...	177	21.25	227.80	12.65	119.8	108.0	6.65	6
4	20.5	...	...	...	22.90	259.43	12.65	136.43	123.0	6.65	6
5	20.5	...	...	89	38.25	515.95	25.16	392.95	123.0	19.16	6
6	20.5	...	...	265	19.13	173.28	8.45	50.28	123.0	2.45	6
7	27.5	6	Adverse.	89	32.30	692.13	25.16	527.13	165.0	19.16	6
8*	27.5	...	Favorable.	...	37.50	692.13	25.16	527.13	165.0	19.16	6
9	27.5	...	...	96	34.60	641.66	23.33	476.66	165.0	17.33	6
10*	27.5	...	Adverse.	...	27.80	641.66	23.33	476.66	165.0	17.33	6
11	34.5	...	Calm.	89	35.30	868.31	25.16	661.31	207.0	19.16	6
12	36.5	8	Favorable.	89	36.50	918.65	25.16	699.65	219.0	19.16	6
13	40.75	...	...	177	26.15	515.70	12.65	271.20	244.5	6.55	6
14	40.75	...	Side.	...	17.70	515.70	12.65	271.20	244.5	6.55	6
15†	40.75	...	Dead calm.	89	31.40	1025.61	25.16	781.11	244.5	19.16	6

* These two experiments were made on the same day, and the second as quickly after the first as the train could be returned to the summit.

† In this experiment the train had not quite acquired an uniform velocity.

These and other ingeniously devised experiments, made with the intent to discover the peculiar manner in which the air operates as a retarding force to

railway trains—but which do not affect the present analysis—led the author to adopt the conclusions given in the subjoined note *.

The fifth conclusion is one which I think the foregoing examination of M. de Pambour's experiments has already negatived, and we shall presently see additional reasons for doubting the authenticity of data which rate the resistance at a still higher sum.

The first part of the eleventh conclusion being founded on the accuracy of the fifth, the important inference thence deduced, must also be considered as negatived, if it be established that the resistance is less rather than greater than has hitherto been supposed.

* "1. That the resistance to a railway train, other things being the same, depends on the speed.

"2. That at the same speed the resistance will be in the ratio of the load, if the carriages remain unaltered.

"3. That if the number of carriages be increased, the resistance is increased, but not in so great a ratio as the load.

"4. That, therefore, the resistance does not, as has been hitherto supposed, bear an invariable ratio to the load, and ought not to be expressed at so much per ton.

"5. That the amount of the resistance of ordinary loads carried on railways, at the ordinary speeds, more especially of passenger trains, is very much greater than engineers have hitherto supposed.

"6. That a considerable, but not yet ascertained proportion of this resistance is due to the air.

"7. That the shape of the front or hind part of the train has no observable effect on the resistance.

"8. That the spaces between the carriages of the train have no observable effect on the resistance.

"9. That the train, with the same width of front, suffers increased resistance with the increased bulk or volume of the coaches.

"10. That mathematical formulæ deduced from the supposition that the resistance of railway trains consists of two parts, one proportional to the load but independent of the speed, and the other proportional to the square of the speed, have been applied to a limited number of experiments, and have given results in very near accordance; but that the experiments must be further multiplied and varied before a safe, exact, and general conclusion can be drawn.

"11. That the amount of resistance being so much greater than has been hitherto supposed, and the resistance produced by curves of a mile radius being inappreciable, railways laid down with gradients of about 16 feet a mile have practically but little disadvantage compared with a dead level; and that curves may be safely made with radii less than a mile; but that further experiments must be made to determine a safe minor limit for the radii of such curves, this principle being understood to be limited to its application to railways intended chiefly for rapid passenger traffic." (*Communicated by Dr. Lardner at the Meeting of the British Association for the Advancement of Science*, 1839.)

It appears from the tenth conclusion that the author considers his experiments, so far as they have gone, as giving results in very near accordance, and demonstrative that the resistance opposed by the air, increases as the square of the velocity. With the view of placing before the eye the degree of this accordance I have composed Table XII., which exhibits all the results fairly deducible from these experiments, excluding any comparisons between those made with a favourable, and others with an adverse, or side wind; comparing, in fact, those only which were affected by like circumstances as to weather. Nor have I compared the results arising out of the data applied to the experiments with different numbers of coaches, as, by the ninth conclusion, the bulk or volume of the mass in motion would appear to affect them. I must also remark that a different amount assumed for friction would alter the ratios of the resistance assigned to the air only, which would be increased or decreased as a higher, or lower rate for that amount were used. According to the manner of estimating the friction of coaches, 5 lbs. per ton would be assigned for some, and even 8 lbs. for others. It will be observed that in each set of comparisons the lowest speed is unity; and I have given the ratios of the direct, as well as of the squares of the velocities, that the eye may readily compare the results with them. The results are set down in the four last columns in the order of the ratios of the velocities, for the more ready appreciation of the degree of accordance which obtains among them.

It cannot fail to be remarked that the term discordance would seem to be much more appropriate than accordance to the indications of the last column of the Table, when referred to the ratios of the squares of the compared velocities in the last column but two. Nor does the mean of all these experiments appear to justify the conclusion that resistance to railway trains from the air is proportional to the square of the speed, for the mean of the 19 comparisons gives a much higher ratio for that resistance, viz. 2.92, the mean square of the velocities being as 1.99 to 1; but no fair average can be struck from such irregular results. The latter part of the author's sixth conclusion seems, therefore, to be more strictly consonant with them than the former part of the tenth.

TABLE XII.

Vide TABLE XI.	Ratio of the velocities.	Ratio of the squares of the velocities.	Ratio of the total resistance per ton.	Ratio of the resistance per ton from the air only.	Ratio of the velocities.	Ratio of the squares of the velocities.	Ratio of the total resistance per ton.	Ratio of the resistance per ton from the air only.
Comparison of Experim[ts]. 1 with 2	1 to 1.08	1 to 1.16	1 to 1.00	1 to 1.00	1 to 1.07	1 to 1.16	1 to 1.00	1 to 1.00
1 ... 3	1.46	2.15	1.84	2.60	1.08	1.16	1.00	1.00
1 ... 4	1.36	1.85	1.84	2.60	1.08	1.17	1.03	1.05
1 ... 5	1.22	1.50	1.07	1.10	1.08	1.17	1.07	1.10
1 ... 6	1.63	2.66	2.76	7.07	1.11	1.23	1.49	2.71
2 ... 3	1.58	2.51	1.84	2.60	1.13	1.28	1.08	1.10
2 ... 4	1.47	2.16	1.84	2.60	1.16	1.35	1.07	1.10
2 ... 5	1.13	1.28	1.08	1.10	1.19	1.44	1.49	2.71
2 ... 6	1.76	3.10	2.76	7.07	1.22	1.50	1.07	1.10
3 ... 4	1.07	1.16	1.00	1.00	1.36	1.85	1.84	2.60
3 ... 5	1.80	3.24	1.99	2.88	1.39	1.94	2.46	4.56
3 ... 6	1.11	1.23	1.49	2.71	1.46	2.15	1.84	2.60
4 ... 5	1.67	2.79	1.99	2.88	1.47	2.16	1.84	2.60
4 ... 6	1.19	1.44	1.49	2.71	1.58	2.51	1.84	2.60
5 ... 6	2.00	4.00	2.97	7.82	1.63	2.66	2.76	7.07
7 ... 10	1.16	1.35	1.07	1.10	1.67	2.79	1.99	2.88
8 ... 9	1.08	1.17	1.07	1.10	1.76	3.10	2.76	7.07
Mean of 8 and 10 ... 11	1.08	1.17	1.03	1.05	1.80	3.24	1.99	2.8 8
12 ... 13	1.39	1.94	2.46	4.56	2.00	4.00	2.97	7.82

Dr. Lardner developed, also, the following particulars of a very valuable experiment with an engine and train, from Liverpool to Birmingham, and back, conducted with much skill and precaution to obtain accuracy in the observation of velocity, and evaporation, by Mr. Edward Woods, engineer to the Liverpool and Manchester railway.

Experiment with the *Hecla* locomotive from Liverpool to Birmingham, and from Birmingham to Liverpool on the same day.

	Tons.
Weight of engine	12
——— tender	10
——— 12 coaches loaded with iron	60
Gross load	82
Useful load	60

PARTICULARS OF ENGINE.

	Inches.
Cylinders	$12\frac{1}{2}$ diameter.
Length of stroke	18

Wheels, 5 feet diameter.

Consumption of coke and water.

	L. to B.	B. to L.	Mean.
Total, coke ......lbs.	3654	3406	3530
Coke per mile. ......lbs.	38.4	35.8	37.1
Water evaporated. ......cubic feet	337	300	318.5
——— ......per mile	3.55	3.16	3.35
Coke per cub. foot of water ......lbs.	10.84	11.35	11.09
Force of water ......lbs.	2510	2234	2372

Total distance traversed 190 miles.

	ho.	min.	sec.
Time.—1st trip, including stoppages	4	28	58
2d Ditto, ditto	4	3	30
	8	32	28
Deduct for stoppages......	2	5	40
	6	26	48
Supposing the 190 miles to be a dead level......	6	8	54

	miles per hour.
Mean velocity during the trips	29.47
Difference for gradients	1.46
Mean velocity on a level	30.93

To reduce this experiment to the terms of the Tables on the author's data, it is necessary to assign a resistance overcome, and for that purpose to ascertain an inclination of plane, or "angle of repose," as he terms it, which would give an uniform velocity of 30.93 miles per hour to the mass. To this end, I have used the mean of experiments 8 and 10, and 11 in Table XI., which alone can be called unobjectionable. The last was made in a calm; the two first were performed as quickly as possible in succession on the same day, with the same coaches, the same wind being favourable to the one, and adverse to the other, so that their resulting mean gives a declivity, and a velocity equivalent to the production of a like effect in a calm, which is for the incline $\frac{1}{92.5}$, and for the speed 32.65 miles per hour. The mean of this last and of experiment 11, is for the incline $\frac{1}{90.75}$, and for the velocity 34.02 miles per hour, which, by inverse proportion, gives the inclination of $\frac{1}{100}$ for the "angle of repose" at a velocity of 30.93 miles per hour. Thus, the resistance or tractive force for the gross load of 82 tons is found, by the author's data, to be 1836.80 lbs. for the mass, or 22.40 lbs. for the ton. On comparing the resistance thus obtained,

with that assigned to nearly similar velocities in Table XI., the correspondence will be found very near; but it is below rather than above the value resulting from the author's method of deducing the resistance, for, according to his ninth conclusion, the bulk or volume of the mass augments the resistance, and the mass drawn by the *Hecla* is greater than that used in the three cited experiments with six coaches, in the ratio of 14 to 6.

The actual mean velocity over the ground in the two trips was 26.95 miles per hour, for which speed I have computed the "angle of repose," by the same process, to be $\frac{1}{114}$, the total resistance 1611.22 lbs. and 19.64 lbs. per ton.

For the purpose of separating the relative quantities of air resistance, and friction, I applied to Mr. Edward Woods, who performed the experiment, whose data for friction were used by Dr. Lardner, and who alone could inform me, from his practical knowledge, of its probable amount for the particular coaches used in this experiment. Mr. Woods states it as about 8 lbs. per ton for those 12 coaches, and I have assumed the same friction for the locomotive and tender; so that, for the velocity of 30.93 miles per hour the resistance of the air would be 14.40 lbs., and for 26.95 miles per hour, 11.64 lbs. per ton. The coaches were loaded with iron weights, and the two trips were performed on the same day, the one immediately succeeding the other.

Mr. Woods's minute and exact observations, during these two trips, will appear from his replies to my queries relative to the diminution of water consumed as steam requisite to be made for waste from delays on the road; an approximation to which necessary correction I could not have arrived at without being informed of the number, and duration of the delays. These are stated by him as follows:

Statement of the time occupied in the trips to and from Birmingham and Liverpool, with the *Hecla*.

	min.	sec.	ho.	min.	sec.
1st Trip.—Liverpool to Birmingham, stoppages included			4	28	58
Time lost on the road.					
1. Getting up speed at Liverpool	2	21			
2. Slackened over Parr Moss	3	17			
3. Stoppage at Warrington	34	48			
4. Ditto Hartford	5	34			
5. Ditto Crewe	7	12			
6. Slackened at Whitmore	1	34			
7. Stoppage at Stafford	8	29			
8. Ditto Wolverhampton	6	0			
9. Slackened at Birmingham	0	24			
			1	9	39
Time which would have been occupied if the train had started from Birmingham at full speed, and travelled from thence to Birmingham without stopping			3	19	19

	min.	sec.	ho.	min.	sec.
2d Trip.—Birmingham to Liverpool, stoppages included			4	3	30
Time lost on the road.					
1. Getting up speed at Birmingham	2	18			
2. Stoppage at Wolverhampton	5	11			
3. Ditto Stafford	5	52			
4. Ditto Whitmore	5	39			
5. Ditto Crewe	6	35			
6. Ditto Warrington	26	16			
7. Delay from overtaking train on the Liverpool and Manchester line	4	10			
			0	56	1
Time which would have been occupied if the train had started from Birmingham at full speed, and travelled from thence to Liverpool without stopping			3	7	29

Dead Stoppages.

First Trip.	min.	sec.	Second Trip.	min.	sec.
Warrington	32	34	Wolverhampton	3	13
Hartford	3	33	Stafford	2	25
Crewe	5	30	Whitmore	2	47
Stafford	6	35	Crewe	3	25
Wolverhampton	4	12	Warrington	24	15
	52	24		36	5

Deducting the dead stoppages, amounting to $88\frac{1}{2}$ minutes, from the whole time, the actual period occupied in travelling was 7 hours, 3 minutes, 59 seconds. The total evaporation was 39812 lbs. effected in $512\frac{1}{2}$ minutes; the proportion for the actual time of the trip, or 424 minutes, 32937 lbs.; difference 6875 lbs. The dead stoppages are divisible into two portions, viz. 9 very short ones comprising 31 minutes, 40 seconds; and 2 long ones equal to 56 minutes, 49 seconds. For the short stoppages I have deducted the full proportion, or 2465 lbs., as the steam would blow on the instant of stopping, perhaps even whilst the engine was coming to rest at the several stations, the regulator being closed some instants before arriving at them.

The two long stoppages at Warrington averaged each 28 minutes, $24\frac{1}{2}$ seconds, but though, at the moment of stopping, the fire would be of the same intensity as during motion, it would be greatly diminished in force at the moment of starting again; for those two delays, therefore, a different correction is required as regards waste of steam. This I have endeavoured to supply.

Mr. Woods has informed me that the usual consumption of coke by engines waiting to start at the termini is about 100 lbs. per hour. This is an useful fact, as it determines, approximately, the difference in the rates of combustion and evaporation by locomotive boilers with the simple draught of the chimney, and with the acceleration given to it by the blast. Their difference on the *Hecla*, at the velocity of the journey, is about 8 to 1. If the evaporation for the whole delay at Warrington were reckoned as proportional to the time, with an active blast, it would amount to 4416 lbs.; and if computed according to the evaporative power of the boiler, without the blast, it would be only 552 lbs. Neither of these can be assumed as the real quantity. I have made a calculation from which I think the true amount of water vaporized in the 56 minutes, 49 seconds, would be very nearly approximated to in the sum of 2650 lbs.; but this may be thought too high, and I have concluded to reckon it as $\frac{1}{3}$d of the active evaporation for the same time, or 1472 lbs., which is equivalent to burning 256 lbs. of coke in the time. Thus, the reduction for waste of water as steam during the delays is $2465 + 1472 = 3937$ lbs.

A further reduction is also necessary for such space as the engine may have passed over without steam in approaching stations, or in descending inclined planes, it being evident that steam would blow off in these intervals.

I found, on applying to Mr. Woods for an approximation with reference to these quantities, that 2500 yards may be deducted, on each trip, for that por-

tion of the ground over which the train passed, by virtue of its acquired impetus, when the steam was shut off whilst approaching the several stations; and that, on each trip, the train descended an inclined plane of a mile and a half in length by gravity alone.—These quantities amount to 30840 feet, and to 7852 cylinder volumes, or to between $\frac{1}{32}$d and $\frac{1}{33}$d part of the 190 miles traversed.

Mr. Woods also informed me, in reply to an enquiry on that head, that "there was a pretty constant simmering of steam from the safety valves during the whole journey." The amount of this portion of waste eludes determination, but, by its amount (which could not be inconsiderable during seven hours) the pressure per volume assigned in the Tables will be too high, as also the consumption of water as steam for the power. This constant waste through the safety valves proves the steam to have been constantly at the blowing off point, and that, when its exit through the cylinders was closed, it escaped freely; consequently, a proportionate deduction must be made for the spaces passed over without steam. By taking the time equivalent to the passage of the train over these 5.84 miles without steam, the quantity of water, ineffective as power from this cause, is found to be 1103 lbs. The effective evaporation, or the weight of water which actually passed through the cylinders, in the shape of steam, is, therefore, $39812-(3937+1103)=34772$ lbs., which quantity stands in the Tables as the consumption of the engine, and is exclusive of waste from the continuous escape of steam through the safety valves.

In estimating the pressure resulting from the comparative volumes of steam and water, $\frac{1}{20}$th is deducted from the water for the useless content of the cylinder passages, and $\frac{1}{32}$d from the volume of steam, for the space traversed without expenditure of power.

The consumption of coke is diminished in the same ratio with that of the water.

The experiment with the *Hecla* resolves itself into two cases:

1st. With reference to the consumption of water as steam, and of coke, applied to the actual mean velocity of the train.

2d. With reference to the consumption of water as steam, and of coke, applied to the velocity assumed by Dr. Lardner to be that which would have resulted, supposing the 190 miles traversed to have been a level plane.

I have computed and set down the results of each of these cases in the

Tables, but I have thought it unnecessary to fill up the columns for useful duty, and power, as the accuracy of the data of resistance is to be judged of from the whole tractive effort required, or gross duty done, compared with the power expended. I can assign no amount for engine friction, or blast-pressure, as the author does not profess to have made any experiments upon the engine to determine these quantities; consequently, no true value can be assigned for the pressure and power arising out of the sum of the resistances. These latter are entered in their respective columns, but they necessarily represent the values of the pressure and power equivalent to the tractive effect, or gross duty only, and have been computed for the sole purpose of comparing them with similar values from the volume test.

On turning to the Tables, and examining the results of this experiment, (case 2,) it will be apparent;

1. That a duty has been performed of double the amount effected by the condensing engine, with an equal expenditure of power. (Column 15.)
2. That, the absolute force impressed upon the pistons, as determined by the relative volumes of water and steam, was 30.95 lbs. per square inch; whereas the tractive effort, requisite to overcome the assigned resistance, amounted to 39.28 lbs. per square inch, exclusive of the force equivalent to the friction of the loaded engine, and blast-pressure. (Columns 29, 30.)
3. That, the power required of the engine to balance the tractive effort alone was 151½ horses; whilst the absolute power furnished by the steam to move the engine, to neutralize the blast-resistance, and to overcome the load, amounted only to 119½ horses. (Columns 33, 34.)
4. That, the water expended as steam per horse-power, per hour, was 37.89 lbs. for the tractive effect or duty only, (column 42); whereas, the condensing engine consumes 70 lbs. per effective horse-power; and, if the two engines were brought to an equality of comparison, the water expended by the locomotive per effective horse-power, per hour, must have been considerably less than 37.89 lbs., as the steam overcame the blast-resistance, in addition to its load.
5. That, compared with a fixed non-condensing engine, at equal pressure, the locomotive—though labouring against the heavy counter pressure of the blast, from which the other is free—is assumed to have performed equal work, with less than one-half the expenditure of power.

6. That, if the resistance assigned by Dr. Lardner as opposed to the progressive motion of the train be correct, the efficiency of the steam in the locomotive, is more than double that obtained by the best condensing engines; more than treble that derived from stationary non-condensing engines; and equal to the performance of a Cornish expansive engine doing a 50 million duty with a bushel of coals.

Such are the incredible results arising out of data, purporting to be fairly and necessarily deduced from unimpeachable experiments. To obtain credence for such results, which, until proved, can be deemed but hypotheses, it is incumbent on those who promulgate and receive them, to explain the process by which the locomotive, commonly considered as the most prodigal of engines, transmutes its apparently enormous consumption of steam into an economy so striking as to shake, if true, all reliance on the fixed and well founded principles by which engineers have hitherto been governed in the application of the steam engine to its various uses.—Until these anomalous and mysterious properties of the locomotive, in its use of steam, be brought to light, and satisfactorily accounted for; or, until a much nearer accordance be established between these hypothetical effects and the ascertained performance of other engines, the resistances assigned as opposed to and overcome by the locomotive, at different velocities, must be regarded by persons conversant with the power of steam, and with the practical capability of engines, as utterly inconsistent with reality, and as resting on no solid foundation. The fallacy of all these data is not only demonstrable by argument, but it is demonstrated by facts furnished by the engine itself, whenever an evaporation approximate even to the truth can be arrived at.

It is important to point out one other error into which railway analysts have fallen, and which is clearly exemplified by the separation of the experiment with the *Hecla* into two cases. I allude to the theoretical methods adopted by them for reducing undulating surfaces to a level.

M. de Pambour extends the length of the Liverpool and Manchester railway from $29\frac{1}{2}$ to $34\frac{1}{2}$ miles, as a compensation for the acclivities, or for the help afforded to his trains in surmounting them by the bank engines, which is the same thing. Dr. Lardner, on the contrary, reduces the actual time of the journey, to that which he assumes would be occupied in performing it on a dead level; this is equivalent to a certain increase of velocity which he assigns, and he supposes the same load to be moved at that increased velocity, with the same power, as at the lower velocity, but he assigns a very different amount of

resistance for each velocity. Now, there is one indubitable measure of the accuracy, or fallacy of these substitutions, viz., the effect produced by the power expended.—In the two cases of the *Hecla* an equal quantity of water as steam has been consumed by the engine, in dragging the same train over the same length of ground; consequently, the power ought to have produced an equal mechanical effect in both cases. How stand the results? By column 29, it is seen that an equal absolute pressure of steam existed on the pistons in both cases, and an equal number of cylinders have been filled with steam and discharged in passing over it; but the time in which the steam was expended is less by 14 per cent. in the 2d case than in the 1st: it is, therefore, certain, that the resulting pressure of the steam upon the pistons could not have been identical in the two cases.

Again, if the resistances assigned at the two velocities were relatively correct—however positively erroneous they might be—the duty done by the same power would be alike in both cases; but this is so far from being the presumed fact, that 12 per cent. more duty has been accomplished at the higher than at the lower velocity. (Column 15.)

The same discrepancy appears on referring to the amounts of absolute and tractive power of the engine; a power, let it be borne in mind, created by an equal expenditure of steam in the two cases. Column 33 shews the difference between the absolute powers to amount to $15\frac{1}{4}$ horses; and column 34 exhibits the same engine, using the same quantity of steam, to have exerted in the 1st case an effort equal to $115\frac{1}{2}$, and in the 2d case, an effort equal to $151\frac{1}{2}$ horses power, or a difference of 36 horses power. No change whatever, be it observed, is supposed to have taken place in the sum total of the forces opposed to the engine, for the same absolute pressure is assumed to have urged the pistons; the friction and blast resistance in the 2d case, (that of the level,) must, therefore, have been nearly if not precisely the same as in the first case; and the conclusion is inevitable that, if the resistance assigned for the load at 26.95 miles per hour be supposed correct, the amount attributed to it at 30.93 miles per hour is very greatly exaggerated; or we must conclude that the method resorted to for reducing the undulating to a level plane is erroneous.

There are numerous practical difficulties against reducing a railway to an imaginary level so as to make the mechanical effects of a given power, on the level, coincide with those obtained under the practical conditions of a railway; and it is manifest that these results ought to agree, if every circumstance con-

nected with the problem be taken into account. If all the resistances and frictions incidental to the engine and its train were truly ascertained for all velocities, and all loads, and corrections were applied accordingly, the effects obtained from a given power moving the same load, at two different velocities, over a level, and over an undulating surface, would be identical. The comparison of these two cases shews the data of resistance to be altogether fallacious; and I think the effects ascribed to a locomotive engine of the dimensions of the *Hecla* will be regarded as fabulous, when it is seen that, in order to accomplish a tractive effort exceeding 150 horses power, the whole force of steam upon the pistons could not have amounted to less than 200 horses; for it cannot be supposed that the friction of the engine so highly loaded, together with the blast-pressure, required less than 12 lbs. per square inch to balance them; which addition would raise the absolute power of the engine to 200 horses. It is, I apprehend, very questionable whether the parts of any locomotive engine yet constructed could hold together, under a total effort of even half that power.

By these observations I am far from presuming to dispute the accuracy of the compensations applied for the acclivities and declivities of the railways traversed in this experiment, as reckoned on the time; but, if the principle on which the level has been calculated be true, the comparison of the two cases establishes the necessity of appreciating other facts than we are at present acquainted with, before it can be demonstrated that a given power will convey a given load at some certain increased velocity along a level, compared with the actual velocity along any given undulating line. This is a practical question of very great import to the science, as well as to the first cost, and ultimate mercantile success of railways, and it is of the utmost consequence that its determination should be sought in a practical sense, and by such practical methods as railways and engines enable us to use. Such a problem as this is not to be solved by deductions from experiments on wagons and carriages running down inclined planes; facts alone can decide it, and they are attainable.

There is no such thing as a constant quantity, in a practical point of view, appertaining to any one of the resistances or frictions which enter into the composition of the sum of the forces opposed to the progressive motion of an engine and train. There can be no doubt but that the air opposes different amounts of resistance to the same body moving through it with different velocities, so that in ascending an acclivity, though the velocity of a train be diminished by the counter force of gravity, the resistance to the train from the air is di-

minished at the same time; and a correction would be required for this quantity at every, even the smallest, deviation of the line from a mathematical level, whether ascending or descending. The pressure of steam upon the pistons varies with the effort required, causing fluctuations in the amount of engine friction and blast-resistances according to the velocity. In the cases under consideration a level has been calculated, and it may be on sound principles, but it does not appear that any corrections have been applied for these varying forces as regards the load, or power; consequently, the theoretical level is a pure fiction with reference to the practical results of the experiment.

The same remarks apply to M. de Pambour's experiments reduced to his level.

I should state, with reference to the amounts of water consumed as steam set down in column 11, for the two cases of the *Hecla*, that the difference arises from having to treat a hypothetical case, viz. that of a level. In the 1st, or actual case, two inclined planes of $1\frac{1}{2}$ mile each occurred, which the engine traversed without steam, besides the 5000 yards in approaching stations, by which the consumption of water had to be diminished $\frac{1}{32.5}$d, as previously explained. The imaginary case of the level requires that the engine should be supplied with steam along the 3 miles deducted for these inclined planes in the 1st case; consequently $\frac{1}{67}$th is the correction in the 2d case for the 5000 yards only. A similar correction for the space passed over without steam was requisite in estimating the volume of steam consumed; as the power was in action in the 1st case, during 61894 double strokes of each piston, and in the 2d case during 62902 double strokes. The mean velocity of the engine and train in both cases is found by dividing the whole distance traversed, by the whole time; but, as the steam did not operate during the whole time in either case, a further correction became necessary for the number of revolutions of the working wheel, or number of double strokes of the piston effected per minute by the steam. In the 1st case the steam operated during 411 minutes instead of the whole time, or 424 minutes; and in the 2d case during 363.4 minutes, instead of 368.9 minutes. Had not these corrections been made, the number of horses power arising out of the assigned resistance, (column 34,) would not have corresponded with the power deduced from the duty, (column 36); and it is evident that the two, being identical, ought to coincide in amount, as they do when they are properly computed.

TABLE of the volume of the steam generated under different pressures, compared to the volume of the water that has produced it.—*New Theory of the Steam Engine by Le Comte de Pambour, page* 76.

Total pressure, in English pounds, per square inch.	Corresponding temperature by Fahrenheit's thermometer.	Volume of the steam compared to the volume of the water that has produced it.	Total pressure in English pounds, per square inch.	Corresponding temperature by Fahrenheit's thermometer.	Volume of the steam compared to the volume of the water that has produced it.	Total pressure, in English pounds, per square inch.	Corresponding temperature by Fahrenheit's thermometer.	Volume of the steam compared to the volume of the water that has produced it.
1	102.9	20954	38	265.3	710	75	308.9	381
2	126.1	10907	39	266.9	693	76	309.9	377
3	141.0	7455	40	268.4	677	77	310.8	372
4	152.3	5695	41	269.9	662	78	311.7	368
5	161.4	4624	42	271.4	647	79	312.6	364
6	169.2	3901	43	272.9	634	80	313.5	359
7	176.0	3380	44	274.3	620	81	314.3	355
8	182.0	2985	45	275.7	608	82	315.2	351
9	187.4	2676	46	277.1	596	83	316.1	348
10	192.4	2427	47	278.4	584	84	316.9	344
11	197.0	2222	48	279.7	573	85	317.8	340
12	201.3	2050	49	281.0	562	86	318.6	337
13	205.3	1903	50	282.3	552	87	319.4	333
14	209.0	1777	51	283.6	542	88	320.3	330
15	213.0	1669	52	284.8	532	89	321.1	326
16	216.4	1572	53	286.0	523	90	321.9	323
17	219.6	1487	54	287.2	514	91	322.7	320
18	222.6	1410	55	288.4	506	92	323.5	317
19	225.6	1342	56	289.6	498	93	324.3	313
20	228.3	1280	57	290.7	490	94	325.0	310
21	231.0	1224	58	291.9	482	95	325.8	307
22	233.6	1172	59	293.0	474	96	326.6	305
23	236.1	1125	60	294.1	467	97	327.3	302
24	238.4	1082	61	294.9	460	98	328.1	299
25	240.7	1042	62	295.9	453	99	328.8	296
26	243.0	1005	63	297.0	447	100	329.6	293
27	245.1	971	64	298.1	440	105	333.2	281
28	247.2	939	65	299.1	434	120	343.3	249
29	249.2	909	66	300.1	428	135	352.4	224
30	251.2	882	67	301.2	422	150	360.8	203
31	253.1	855	68	302.2	417	165	368.5	187
32	255.0	831	69	303.2	411	180	375.6	173
33	256.8	808	70	304.2	406	195	382.3	161
34	258.6	786	71	305.1	401	210	388.6	150
35	260.3	765	72	306.1	396	225	394.6	141
36	262.0	746	73	307.1	391	240	400.2	133
37	263.7	727	74	308.0	386			

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TABLE VIII.

No. of experiments.	LOCOMOTIVE ENGINES.	Diameter of cylinders.	Length of stroke.	Diameter of working wheels.	Weight of gross load.	Weight of useful load.	Length of trip.	Duration of trip.	Mean velocity of engine per hour.	Consumption of water during the trip.	Consumption of coke during the trip.	Resistance at 8 lbs. per ton on the load. Gross load.	Resistance at 8 lbs. per ton on the load. Useful load.	Duty in pounds raised 1 foot. By 1 lb. of water as steam. Gross duty.	Duty in pounds raised 1 foot. By 1 lb. of water as steam. Useful duty.	Duty in pounds raised 1 foot. By 1 lb. of coke. Gross duty.	Duty in pounds raised 1 foot. By 1 lb. of coke. Useful duty.
	Experiments by M. De Pambour. (Treatise on Locomotives, 1836.)	Inches.	In.	Ft.	Tons.	Tons.	Miles.	Minutes.	Miles.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
I.	Atlas	12	16	5	206.90	190.00	$29\frac{1}{2}$	182	9.72	8260	1596	1655.20	1520.00	31212	28662	161707	148499
II.	Atlas	...	...	...	139.54	122.64	$29\frac{1}{2}$	118	15.00	5937	1224	1116.32	981.12	29287	25741	142095	124887
III.	Atlas	...	...	...	52.05	35.15	$34\frac{1}{2}$	114	18.15	5524	881	416.40	281.20	13731	9273	84961	58142
IV.	Atlas	...	...	...	42.20	25.30	$34\frac{1}{2}$	86	24.07	4558	720	337.60	202.40	13492	8088	85412	51207
V.	Vesta	$11\frac{1}{8}$	...	...	41.16	28.15	$34\frac{1}{2}$	65.5	31.60	4130	774	334.88	225.20	14770	9932	78813	53000
VI.	Vulcan	11	...	...	47.41	34.07	$34\frac{1}{2}$	77	26.90	4646	664	379.28	272.56	14870	10686	104050	74773
VII.	Leeds	...	...	...	95.41	83.34	$29\frac{1}{2}$	95	18.63	5989	897	763.28	666.72	19851	17339	132541	115668
VIII.	Leeds	...	...	...	44.08	32.01	$34\frac{1}{2}$	77.5	26.70	5317	690	352.64	256.08	12081	8773	93096	67603
IX.	Fury	...	...	...	57.00	43.80	$34\frac{1}{2}$	95	21.79	5446	746	456.00	350.40	15252	11720	111332	85561
X.	Fury	...	...	...	64.36	51.16	$34\frac{1}{2}$	90	23.00	4878	806	514.88	409.28	19206	15283	116365	92499
XI.	**Experiments by Mr. Robert Stephenson.**	12	18	...	220.00	204.75	14	60	14.00	4812	616	1760.00	1638.00	27036	25162	211200	196560
XII.	(Tredgold on the Steam Engine, 1838. 2d ed.)	...	...	...	40.00	24.75	35	60	35.00	4812	616	320.00	198.00	12289	7604	96000	61023
	From data by M. De Pambour. (Theory of the Steam Engine, p. 34; 1839.)																
XIII.	Leeds	11	16	...	95.41	83.34	20.34	60	20.34	3026	...	782.00	726	27753	25765	...	...
XIV.	Leeds	...	...	...	45.59	33.52	29.09	60	29.09	3026	...	177.00	166	8983	8425	...	...
	From experiments reported by Dr. Lardner to the British Association for the Advancement of Science, 1839.																
XV.	Hecla 1.	$12\frac{1}{2}$	18	...	82.00	60.00	190.00	424	26.95	34772	6064	1611.22	...	46484	...	266552	...
	Hecla 2.	...	...	...	82.00	60.00	190.00	368.9	30.93	35340	6164	1836.80	...	52141	...	298941	...
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18

TABLE IX.

No. of experiments.	Mean velocity of the engines per minute.	Mean velocity of the pistons per minute.	Ratio of the velocity of the working wheel and piston.	Area of both cylinders.	Length of double stroke	Capacity of both cylinders per double stroke.	Number of revolutions of the working wheels or, No. of double strokes of the pistons per minute.	Volume of steam consumed per minute	Volume of water consumed per minute.	Volume of steam to 1 of water.	Pressure per square inch on the pistons, above the atmosphere, deduced from the volume of steam.	Pressure per square inch on the pistons, above the atmosphere, deduced from the sum of the resistances.	Difference of pressure on the pistons deduced from the volume of steam compared with that from the sum of the resistances.	Pressure per square inch in the boilers above the atmosphere.
	Feet.	Feet.	Ratio.	Sq. in.	In.	Cub. ft.	No. per min.	Cub. ft.	Cub. ft.	Vol.	lbs.	lbs.	lbs. per sq. in.	lbs.
I.	855.82	145.32	5.892	226.00	32	4.185	54.51	228.12	0.517	441	49.29	49.77	− 0.48	53.7
II.	1320.00	224.13	...	...	...	...	84.07	351.83	0.573	614	29.79	34.00	− 4.21	53.0
III.	1597.80	271.85	...	...	...	...	101.97	425.90	0.552	771	20.00	13.49	+ 6.51	53.0
IV.	2118.13	359.67	...	...	...	...	134.91	564.59	0.605	933	13.29	11.17	+ 2.12	54.5
V.	2781.06	472.22	...	194.40	...	3.600	177.13	637.66	0.719	886	15.30	14.68	+ 0.62	51.0
VI.	2365.72	401.71	...	190.06	...	3.515	150.68	529.64	0.688	770	20.04	15.10	+ 5.94	54.5
VII.	1639.57	278.41	...	...	...	...	104.43	367.07	0.729	510	39.19	27.97	+11.22	54.0
VIII.	2350.45	399.11	...	...	...	...	149.71	526.23	0.782	672	25.62	13.66	+11.96	49.0
IX.	1917.47	325.59	...	...	...	...	122.13	429.28	0.653	657	26.62	16.97	+ 9.65	59.0
X.	2024.00	343.67	...	...	...	...	128.91	453.11	0.618	733	21.95	19.03	+ 2.92	60.0
XI.	1232.00	235.41	5.238	226.00	36	4.708	78.47	369.43	0.914	404	55.69	47.54	+ 8.15	50.0
XII.	3080.00	588.51	...	...	...	...	196.17	923.56	0.914	1010	11.10	10.00	+ 1.10	50.0
XIII.	1789.92	303.92	5.892	190.06	32	3.515	114.00	400.71	0.770	520	38.50	38.43	+ 0.07	54.0
XIV.	2559.92	432.98	...	...	...	...	162.41	570.87	0.770	741	21.54	23.93	− 2.39	54.0
XV.	2366.03	451.70	5.238	245.43	36	5.113	150.59	746.37	1.246	599	31.04	34.38	− 3.34	60.0
	2719.43	519.17	...	...	...	...	173.09	871.82	1.453	600	30.95	39.28	− 8.33	60.0
	19	20	21	22	23	24	25	26	27	28	29	30	31	32

Back of
Foldout
Not Imaged

TABLE X.

No. of experiments.	Total horse power deduced from the pressure per volume of steam.	Horse power deduced from the pressure of the sum of the resistances.	Effective horse power.	Tractive horse power deduced from the gross duty.	Useful horse power deduced from the useful duty.	Horse power arising from the difference of pressure on the pistons, deduced from the volume of steam, compared with that from the sum of the resistances.	Water consumed per hour.	Coke consumed per hour.	Consumption of water as steam per horse power per hour. Per horse power from the volume of steam.	Per horse power from the sum of the resistances.	Per effective horse power.	Per tractive horse power.	Per useful horse power.	Consumption of coke per horse power per hour. Per horse power from the volume of steam.	Per horse power from the sum of the resistances.	Per effective horse power.	Per tractive horse power.	Per useful horse power.
	H. P.	H. P.	H. P.	H. P.	H. P.	H. P.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
I.	48.93	49.41	......	42.92	39.41	— 0.48	2723.07	526.15	55.65	55.11	......	63.44	69.09	10.75	10. 6	......	12.25	13.35
II.	45.62	52.07	......	44.65	39.24	— 6.45	3018.81	622.37	66.17	57.97	......	67.61	76.93	13.64	11.95	......	13.93	15.86
III.	37.06	25.00	......	20.16	13.61	+12.06	2907.36	463.68	78.45	116.02	......	144.21	213.61	12.51	18.54	......	23.00	34.07
IV.	32.54	27.35	......	21.66	12.99	+ 5.19	3180.00	502.32	97.92	116.27	......	146.81	244.80	15.43	18.36	......	23.19	38.67
V.	42.52	40.80	......	28.22	18.90	+ 1.72	3783.18	709.16	88.97	92.72	......	134.06	199.21	16.67	17.38	......	25.12	37.34
VI.	46.18	34.80	......	27.19	19.53	+11.38	3620.25	517.40	78.39	104.03	......	133.14	180.24	11.20	14.86	......	19.02	26.49
VII.	62.67	44.73	......	37.61	33.12	+17.94	3782.52	566.52	60.35	84.56	......	100.57	114.20	9.03	12.66	......	15.06	17.10
VIII.	58.72	31.31	......	25.11	18.23	+27.41	4116.38	534.19	70.10	131.47	......	163.93	225.80	14.53	17.06	......	21.27	29.29
IX.	49.77	31.73	......	26.49	20.36	+18.04	3439.57	471.18	69.10	108.40	......	129.84	168.93	9.46	14.85	......	17.78	23.14
X.	43.33	37.57	......	31.57	25.10	+ 5.76	3252.00	537.33	75.05	86.55	......	103.00	129.56	12.40	14.30	......	17.02	21.40
XI.	88.66	76.64	......	65.70	61.15	+13.03	4812.00	616.00	53.60	62.78	......	73.19	78.07	6.86	8.03	......	9.37	10.73
XII.	44.73	40.30	......	29.83	18.46	+ 4.43	4812.00	616.00	107.35	119.40	......	161.17	260.69	13.77	15.28	......	20.65	33.36
XIII.	67.24	67.11	49.65	42.41	39.37	+ 0.13	3026.00	......	45.00	45.09	60.00	71.35	76.89	......	......	......	......	......
XIV.	53.55	59.50	35.42	13.73	12.87	— 5.95	3026.00	......	56.50	50.85	85.43	220.39	235.12	......	......	......	......	......
XV. 1	104.29	115.51	......	115.52	......	—11.23	4920.56	858.11	47.18	42.59	......	42.59	......	8.22	7.42	......	7.42	......
XV. 2	119.52	151.68	......	151.36	......	—32.16	5747.89	1002.54	48.08	37.89	......	37.97	......	8.38	6.60	......	6.62	......
	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50

OF MOMENTUM AS A MEASURE OF THE EFFECT OF LOCOMOTIVE ENGINES.

The effective power of a locomotive engine—by which is meant the excess of power after overcoming its proper friction, and the resistance from the blast—is solely expended in the generation of momentum. The momentum communicated to the entire mass set in motion represents the useful mechanical effort exerted by the steam; this effect of the engine is, therefore, at all times determinable; for, being the simple product of the mass moved, multiplied into its velocity, it is the product of two quantities easily ascertained under all the practical circumstances of railway traffic. The consumption of power, as water in the shape of steam, is a third quantity also readily appreciable.

Were it possible to work a locomotive engine and its train *in vacuo*, on a truly level plane, the momentum generated by an equal expenditure of power would be a constant quantity at all velocities; for, the resistance being invariable, equal momenta would be produced by an equal expenditure of power with all loads, as the velocity attained would be in the inverse ratio of the loads, and vice versâ. This hypothetical case supposes friction and resistance of all kinds to be constant. We know, however, in practice, the power and velocity of an engine and train to be limited by the interference of numerous impediments, which may be enumerated as follows:

1st. The air—the amount of resistance or assistance from which source, must be nearly as inconstant as the wind itself—has to be displaced; a passage has to be forced through it; but, in what ratio this obstacle varies with the velocity of a train, or in what proportion it enters into the total sum of the resistance to be overcome, at any one velocity, we are at present utterly ignorant.

2d. The friction of a body moving upon an inflexible uniform plane is commonly assumed to be the same, or nearly so, at all velocities for equal weights, but it is questionable whether this assumption be well founded in its application to railways, as they are very far from being inflexible uniform planes, but consist, on the contrary, of a series of short elastic bars, very imperfectly united, with points of union which offer sensible obstructions to a train passing over them. No determinate experiments have

yet acquainted us with the value of this friction at any one velocity; nor do we know whether it increases or diminishes, or in what ratio with the speed and weight of a train. The practical condition of a railway is also at no time so perfect as to present, for many yards successively, either a true rectilinear or a true transverse level; circumstances which—inasmuch as they vary greatly in different parts of the same railway, and still more in different railways—must affect the amount of friction.

3d. The blast-pressure—which is actually, though not intrinsically, a condition of the engine—opposes to the power amounts of resistance fluctuating with the load, and with the velocity. Its intensity may be supposed to have some definite relation to that of the active steam, and to the number of discharges from the cylinders which have to be made in a given time; but its value is, at present, unascertained for any one load, at any one velocity.

4th. The friction of the moving parts will be a constant quantity for the same engine, without load, and in good order; but it necessarily varies for engines of different dimensions and weights, and according to the method of applying the power, number and diameter of the wheels, &c.

5th. The additional friction brought upon the moving parts of engines by their load, and by the velocity at which they are travelling, is also a fluctuating quantity, and one which it is extremely difficult to ascertain.

Such is the inconstant nature of the principal forces which have to be overcome by the power applied to a locomotive engine. All attempts to determine their special as well as their total value have proved fruitless, and the widely discordant estimations assigned by the experimenters whose labours have been examined, exhibit the difficulties of arriving at the true determination of some of these forces, to be undiminished, if not insurmountable.

Until the value of the blast-pressure be definitely ascertained, no true estimate of the effective power of an engine employing it can be made; consequently, the force of such engine cannot be measured by the consumption of water as steam; for, all the power employed to overcome the resistance of the blast is ineffective. Until, therefore, this particular resistance be determined for every pressure on the piston, no quantity of water as steam can be correctly assigned as the measure of an effective horse-power in a locomotive; nor can we accurately compare its effects with those of other engines. All we do know is that, by that amount, the locomotive must expend more

steam for equal effects, than a fixed non-condensing engine, at equal pressures, if their friction be the same.

A knowledge of the momentum generated by an equal expenditure of water as steam in equal times, furnishes a true test of the useful mechanical performance of the engine, as well as a true test of the difference of power consumed in the production of equal momenta, or of the same useful mechanical performance at different velocities; for, the expenditure of steam varies only according to the absolute resistance opposed to it, which it exactly balances, and, therefore, accurately measures. The momentum of the mass moved being a measurable product of the power, and this product diminishing with every increase of velocity, in the same ratio that the increments of resistance opposed to the acquisition of such higher velocity augment, the quantity of power employed to generate equal amounts of momentum becomes a true measure of the increased resistance overcome by that power, from whatever sources it may arise. These principles are embraced in the two following propositions:

Proposition 1.—Equal momenta would result at all velocities from an equal amount of power expended in equal times by the same engine, if the forces opposed to progressive motion, and to the effective use of the steam in the engine, were uniform at all velocities.

Proposition 2.—The difference between the momenta generated by an unit of power in a given time at various velocities, measures the difference in the sum of the resistances opposed to the power at those velocities.

By ascertaining, therefore, three facts which are at all times within our reach, viz. the gross weight of an engine, tender and train—their mean velocity—and the expenditure of water as steam during the trip, very simple computations inform us;

1st. Of the mechanical effect realized by a given power at all velocities.

2d. Of the total increase or decrease of resistance at all velocities.

3d. Of the ratios which the increase or decrease of resistance, at different velocities, bear to the ratios of those velocities.

Two other results, distinct in their nature from the foregoing, are disclosed by a knowledge of the same three facts, viz. the amount of gross and useful tractive effect realized by an equal expenditure of power at all velocities. These form what may properly be termed the commercial results, of which the latter is the only useful quantity. The difference between these two

amounts is an useless quantity in a practical sense, and is a costly waste of power incident to the locomotive functions of the engine and tender, over and above the waste arising from the unascertained and ineffective portion of the whole power required for blast*.

Tables XIII. and XIV. contain the reductions of a large assemblage of results to the terms necessary for the exhibition, and development of these views. They comprise forty-nine experiments, being all those previously referred to, with the addition of the experiments by Mr. Nicholas Wood on the Great Western and London and Birmingham railways, and others. To these experiments I have added (Table XIII.) the dimensions of the engines, that particulars, so necessary for every investigation into the performance of the locomotive, may not be wanting for any one engine referred to in this paper.

Table XIII. registers the velocity of the engines, the consumption of water as steam, the loads, the absolute momenta per second, the momenta generated by equal power in equal times, viz. by 1 lb. of steam per second—also the weights of the gross and useful loads moved by equal power, viz. by 1 cubic foot of water as steam, at the velocity of each experiment. The water stated in columns 4 and 12 is the whole quantity expended during the trips, inclusive of all losses which may have taken place, excepting when corrections have been supplied, as in the case of the *Leeds* (*new data,*) and *Hecla*. The water assigned as the consumption of the *Atlas*, experiment III., is computed proportionally to the other experiments on that engine, M. de Pambour not having noted it, and it being important to have the means of comparing two pairs of results with this engine, on the actual and assumed distances of 29½ and 34½ miles, separately, as false conclusions only could arise out of a comparison of the real with the fictitious distance traversed†.

* These two losses constitute the principal difference in the economy of power between stationary and locomotive engines, as instruments of railway traction; the former having to suffer a deduction for loss of power arising from the use of the rope. The precise relation of the two engines in the production of useful effect by an equal expenditure of power, offers many interesting points for investigation, which have latterly been little enquired into. Many important facts connected with railway science, and with the profitable adaptation of railways to other than rapid passenger traffic, require to be, and could not fail to be illustrated by a well arranged, definite series of experiments on both engines.

† For the comparative purposes of these tables, the actual evaporation from the boilers can only be used as data, unless every one of the experiments had been supplied with accurate corrections for waste. As they stand in these tables, we have the quantities of water practically expended as power, with which it is necessary to rest contented until experiments be conducted

Column 11 exhibits the momentum, or product of the mass, in tons, of the engine, tender, and train multiplied into its velocity in feet per second; and the sums thus represent the respective mechanical effect produced per second by each engine. If precisely equal quantities of water as steam had been expended in equal times, the sums in this column would have accurately shewn the relative effects realized by the expenditure of an equal force at the different velocities, and with the different loads; but the consumption of a given power, in a given time, formed no condition of the experiments, nor could it be practically accomplished. In order, therefore, to obtain a true expression of the effect in terms of power, it is necessary to find the momentum proportionate to each unit of power expended in its generation. The unit chosen is one pound of water as steam. These results are accordingly given in column 12, and the effect of a pound of water as steam—or a pound of power—at any one of the velocities recorded in column 3, is strictly comparable with a like effect, at any other velocity, by the notation adopted in column 12.

The series of results in this last column serves, also, to a certain extent, as a test of accuracy in the conduct of the experiments, for we know that the same mechanical effect cannot be realized at a high, as at a low velocity, by the same engine; we know that an engine dragging a load at 23 miles per hour, must consume more power, to acquire an equal momentum, than at a lower velocity. A reference to the *Fury* (previously adverted to as giving anomalous results,) exhibits that engine as having performed more work at 23, than at $21\frac{3}{4}$ miles per hour, by the ratio of 24 to 19; it is, therefore, with certainty

with such care and precision as to require no corrections, or such only as are, in their nature, unavoidable. These quantities fairly represent, perhaps, the comparative consumption of power for given effects on the different railways, as we cannot but suppose the engines, when under charge of the parties who conducted these experiments, to have been managed with greater care than when left to the sole guidance of the attendant engineer, as in common practice. Mr. Wood has assigned no value for loss of steam by the safety valves, delays, or other sources, so that any attempt to determine the positive power of those engines would be fruitless; and if M. de Pambour's data for water were corrected, and the others uncorrected, the comparison of effects resulting from an equal expenditure of power by the different engines, on the different railways, would be entitled to no credit. If the evaporation in M. de Pambour's experiments were diminished by one-fourth, according to his instructions, or by any other equal amount, the relative comparisons between his engines would still hold good; but it is evident that the relation between his results and others would be altogether faithless, unless it were certain that the latter required no correction, or unless an ascertained correction were supplied.

we may conclude one or both of those experiments to be erroneous, though we cannot determine which of them, for want of a sufficient series of results from the same engine to guide our judgment. For subsequent comparison, therefore, I have struck out these two experiments as unsatisfactory.

The *North Star* affords a sequence of six experiments at velocities varying from $18\frac{1}{2}$ to $38\frac{1}{2}$ miles per hour, but the sequence of results is so irregular as to indicate error in two of them, which I have accordingly marked and rejected, for it is certain that a greater measurable effect must accrue from the expenditure of equal power at 25 than at 30, and at $31\frac{1}{2}$ than at 34 miles per hour; yet, the reverse appears on the face of the experiments. It is also equally impossible that a greater momentum should have been generated by a like consumption of force at 34, than at 25 miles per hour.

The seven experiments on the *Æolus* present a pretty uniformly descending gradation in the momenta resulting at speeds between $17\frac{3}{4}$ and $33\frac{1}{4}$ miles per hour, excepting Experiment II., which must be discarded as erroneous.

Evidence of accuracy is satisfactorily developed by other trials, the selection of one or two examples of which will serve to illustrate, and direct attention to the value, and searching nature of this test. Experiments I. and II. on the *Neptune*, at velocities almost identical, but with a load somewhat greater in the second than in the first, shew an accordance in their results so close as to merit confidence in the conduct of them. The two experiments on the *Lion* are even more conclusive, the velocities being nearly identical, yet it is seen that power was consumed in exact proportion to the loads, though they varied by 25 per cent.; and, consequently, that the same measurable effect, or equal momenta, resulted from the consumption of equal power at similar velocities. These experiments exhibit the great importance of a repetition of trials at like velocities with like, or even unlike loads, to inspire perfect confidence in their results. The four experiments on the *Apollo*, as also those on the *Harvey Combe*, and *Bury's engine*, afford rational grounds for considering them to be respectively entitled to credit, inasmuch as the descending scale of their effects comports with the ascending scale of their velocities. The same remark applies to M. de Pambour's series, with the exception of the *Fury*.

M. de Pambour has noted the state of the atmosphere during his experiments, none of which appear to have been affected by any particular force of wind either favourable, adverse, or transverse. I find no mention made by Mr. Wood of any extraordinary derangement from this source, but it must be

understood, as before remarked, in citing these experiments as worthy of confidence, that expression extends only to the mutual comparison of their results, and involves the supposition that the correction for loss of steam from waste, delays, &c., would be pretty much the same, for the same engine, in each experiment.

Table XIV. is a summary of the ratios of the velocities, and of their squares, brought into juxta-position with the ratios of the power expended to produce equal momenta, equal gross, and equal useful effects, by the comparison of pairs of experiments on the same engine given in Table XIII., and referred to in column 2. The actual velocities of each contrasted pair are reproduced in column 3, it being important to observe, not only the ratio of the velocities, but the particular speed of the compared engines. The lowest velocity of each pair is considered as unity. The influence which velocity exerts over the expenditure of power for equal mechanical, and equal commercial effects, is clearly manifested upon each engine in columns 6, 7, 8; and the amount of loss, attributable to the increase of resistance at the higher velocities, is shewn in the last three columns.

Four Means are derived from these results,

Mean 1. as explained in the table, embraces the whole number of experiments entitled to confidence in Table XIII., and this large average—which the results of 42 experiments upon 15 different engines may be considered—informs us;

1st. That when velocity is increased in the ratio of 1.52 to 1, an increased consumption of power is required for the production of equal mechanical effects, or of equal momenta, in the ratio of 1.43 to 1, being somewhat less than in the direct ratio of the velocities.

2d. That power is expended in the ratio of 2.43 to 1, or in about that of the squares of the velocities to produce equal gross commercial results.

3d. That power is expended in the ratio of 3.11 to 1, or in not much less than that of the cubes of the velocities, to realize equal useful commercial results.

In other words, it appears, from the mean of all these experiments, that a loss of 68, 59, and 30 per cent. on the useful, gross, and mechanical effects, respectively, arises from an equal expenditure of power at a velocity of 30.92 miles per hour, compared with a velocity of 20.32 miles per hour. It is important to have an exact method of arriving at such general results as these, but it is of still

greater consequence to be able to test, and ascertain the relative qualifications of particular engines, and the relative perfection of different railways; to the latter purpose I will first apply the powers of this method of analysis.

The Means 2, 3, 4, afford special results from these experiments on three railways, which distinctly mark the order of economy to be, 1st. Liverpool and Manchester; 2d. London and Birmingham; 3d. Great Western; as developed by the realization of the greatest mechanical effect, and of the greatest useful commercial effect, from an equal expenditure of steam power. It is, however, incumbent upon me to guard against these conclusions being considered as other than the indications of results flowing from the particular experiments which are here collected together; experiments which can neither be considered so numerous, nor so exact, as definitive determinations of so important a nature require. The Liverpool and Manchester railway has undergone certain changes in the weight of its rails since M. de Pambour's experiments; the engines may also be of other dimensions, and, for aught I know, more or less economical of power. Two engines only were subjected to trial by Mr. Wood on the London and Birmingham railway, and only over a small part of it. The Great Western railway was incomplete, the engines of a rather novel construction, and, at the time, somewhat recently put to work. It is, therefore, to be understood that a method of investigating the economic qualities of locomotive engines, and of railways generally, is here alone attempted to be illustrated, not the actual state of perfection of any one engine, or of any one railway, for which purposes I can only, at present, use the materials furnished to my hand.

The Means in Table XIV. are insufficient for either of these objects, as a true comparison of the qualities of engines requires that their effects should be manifested, as nearly as possible, at similar velocities; for, we are ignorant whether the resistances increase by the same ratios at low, as at high velocities: careful and repeated practical experiments can alone determine whether the rate in which resistance augments be the same, for example, between 10 and 20, as between 20 and 40 miles per hour.

For the more perfect exhibition of this method of analysis, and for the most comprehensive development of its powers afforded by the recorded experiments, Table XV. has been composed, which contrasts the results of six experiments on the Great Western, with six on the London and Birmingham railways, at velocities as nearly as possible identical. The power of the engines, at the mean speed of 27 miles per hour is represented in column 2, by the gross loads

respectively dragged at that speed; and it appears from the ratios of the loads, that the London and Birmingham engines were less powerful than those of the Great Western by 24 per cent.; but, by column 3, we learn that though the Great Western engines were more powerful—as they ought to be from their dimensions—the London and Birmingham engines performed 18 per cent. more work with an equal consumption of power.

The weight of useful load dragged is also in favour of the Great Western by 19 per cent., (column 4,) but column 5 shews that useful load to be accomplished by a considerable sacrifice of power, and, therefore, by a greater expense than an equal useful load on the London and Birmingham; the difference being 27 per cent. in favour of the latter railway. These are the commercial results incontestibly indicated by the comparison of the loads, and consumption of power assigned to the engines; but, before proceeding further in the investigation, I may observe that these results do not necessarily prove the system adopted on the one railway to be more or less economical than the other; as in a question of such magnitude, other important considerations and details have to be taken into account. It may happen to be a matter of extreme consequence to the interests of one railway to have a power at command capable of conveying a much larger number of passengers at one time, than another railway, and though this object may not be effected without a sacrifice of power by an individual engine, it may suit the necessities of that railway, and be cheaper than employing more engines, and more trains. I know not whether of the two, the Great Western or London and Birmingham, require to drag the heavier net load—a fact apart from this investigation—but we see that in order to gain an increased amount of useful load of 19 per cent., 27 per cent. more power was absorbed by the Great Western's engines.

The true comparison of the effective performance of the engines on the two railways is exhibited in column 6, by which it appears that the mechanical effect of the *Harvey Combe* and *Bury's engine*, was greater, from an equal expenditure of power, by 18 per cent., than that obtained from the engines compared with them.

Supposing a fact of this nature to be established by experiments of unexceptionable authenticity, the question arises as to its cause or causes, a question seriously involving the interests of existing, and of future railways. The enquirer, after satisfying himself that the engines were equally well constructed, and the power equally well applied, would naturally and necessarily be led to

examine the difference in the gauges of the rails, and in the weight of matter uselessly put in motion on the compared railways; an increase in the latter being an effect consequent to the adoption of a wider gauge, and the width of gauge itself influencing the resistance to progressive motion. I have given in column 7 the mean weights of the engines and tenders under comparison, and in column 8 the widths of the gauge of the two railways. The ratios of these are also given, as for the other facts, considering the Great Western to be unity, and the comparison shews that, with a gauge 34 per cent. less in width, the two London and Birmingham engines realized an equal mechanical effect with 18 per cent. less consumption of power: it is also seen that they realized 27 per cent. greater useful effect; and, by turning to column 7, it appears that they had 38 per cent. less useless load to drag, on that portion of the gross load composed of the weight of engine and tender. The conclusion, therefore, would be —on the supposition that no doubt existed as to the correctness of the experiments, and that they were sufficiently numerous, and sufficiently varied—that the difference in effect was produced by the conjoint operation of the width of gauge, and increased weight of useless mass.

Supposing these facts to be indisputable, or to be granted for the sake of argument, the particular values due to the increased resistance, and increased useless load, as separate quantities, are discoverable and assignable. Assuming, for this purpose, the highest results (those obtained on the London and Birmingham) to be unity, it appears that by increasing the width of gauge one half, or 50 per cent., the weight of engines and tenders—or the useless mass—is increased 59 per cent. on the Great Western. The acquisition of momentum, or of measureable mechanical effect, would, in no degree, be influenced by this change of weight; but, it is clear that some cause or causes operated to diminish this effect, for column 6 shews that the Great Western engines, with an equal expenditure of power, realized 16 per cent. less momentum—or, what is the same thing, had to overcome 16 per cent. greater resistance—than the engines on the London and Birmingham, at equal velocities. Now, it appears by Mr. Wood's Report that, of the portion of the two railways respectively traversed by the engines, the Great Western approached the nearest to a level; and, consequently, that the additional resistance encountered by the Great Western's engines, must spring from some other, and some very influential source. It would, therefore, from the facts before us—and on the assumption that all the engines compared were, mechanically speaking, equally perfect—

seem to be a fair conclusion that a width of gauge increased by 50 per cent. over another width, augments resistance to progressive motion by 16 per cent.; and that by reason of such additional resistance, and of the additional useless load caused by the width of gauge, the commercial or useful tractive effect, for equal power, is diminished by 22 per cent.; a conclusion which attributes 16 per cent. as loss arising from increased opposing force, and 6 per cent. as loss arising from the increased useless mass of matter put in motion.

Particular instances might be selected which would exhibit a much larger difference than the foregoing between the effects of engines on these two railways; as, for example, Experiment VI. of the *Æolus*, and Experiment II. of the *Apollo*, with loads and at velocities nearly identical, and giving nearly similar results, compared with Experiment II. of *Bury's engine* at the same speed, viz. 29.8 miles per hour, shew an advantage in favour of the latter of 50 per cent. on both the mechanical, and commercial effects. The results of Experiment II. on the *Venus*, compared with Experiment III. on the *Harvey Combe* at velocities somewhat greater than the last cited, are also in favour of the London and Birmingham engine, though in a less ratio, being 10 per cent. on the mechanical, and 30 per cent. on the useful effects. These instances are adduced simply as confirmatory of the mean in Table XV., though single comparisons, unsupported by repeated trials at the same velocity, are not to be regarded, as indicative of the degree of economy, with the same trust as an average from numerous experiments.

The respective economic qualities of engines are clearly shewn by the last column of Table XIII. The *Neptune*, Experiments I. and II., exhibits the acquisition of a higher sum of momentum, with equal power, at $23\frac{1}{2}$ miles per hour, than the *Lion* at $22\frac{1}{2}$. The *Neptune*, also, exceeds the *Premier* on the mean of experiments at similar velocities. The *Vulcan* performed considerably better work at 26.90, than the *Leeds* at 26.70 miles per hour; the *Æolus* and *Apollo* gave equal results at 29.8 miles per hour; the *Venus* exceeded the *Æolus* at 22.50 miles per hour, &c.

It will be apparent from Table XIV. that general conclusions cannot be safely drawn from the effects of one or two particular engines. It might be deduced from comparisons on the *North Star* alone, that the sum of resistance increases in a much higher ratio than that of the squares of the velocities; whilst the *Æolus*, on the same railway, exhibits the resistance—as do most of the other comparisons—as augmenting in a ratio somewhat less than that of

the direct velocities. These indications should point attention to the mechanical structure of the engines, provided the enquirer be confident of the accuracy of his data, and that his experiments were satisfactory in other respects—such as their being made over precisely the same ground, on the same day, and as nearly as possible at the same time—experimental conditions absolutely essential for determining the relative excellence of different locomotive engines. Supposing these conditions to have been observed, and the recorded results to have been elicited, it might be inferred that the action of the *North Star* was somewhat imperfect at the time of trial—that its blast-pipe was too contracted —or that some other imperfection caused the consumption of power to be unduly increased at the higher velocities. Comparison 6 and 1 on the *Æolus*, at velocities very little varying from those of 4 and 1 on the *North Star*, shew the former engine to have greatly exceeded the latter in economy of power, and in the production of useful effect with equal power.

Other comparisons shew the *North Star* to have been by far the most powerful engine on the list—but, in most cases, prodigal of power. Experiment I., column 11, Table XIII., exhibits this engine to have realized an absolute mechanical effect of more than double that by *Bury's Engine*, Experiment I., at nearly similar speeds; but column 12 shews that, for equal effect, the former consumed the most power, though at the rather lower speed of the two. Again, the *North Star*, (same experiment,) performed nearly four times the work of the *Atlas*, Experiment III., at equal velocities, and in this instance, with less power for equal mechanical effect *.

Table XV. shews the mean velocity of the three experiments on the *Harvey Combe*, and the three experiments on *Bury's Engine* to be nearly identical; the particular speeds of each trial were also nearly similar. The mean of the three results of each, (column 12, Table XIII.,) shews *Bury's Engine* to have produced 10 per cent. greater mechanical effect than the *Harvey Combe*. This may be called a small difference, but, if the experiments are trustworthy, that small differential effect must have had a cause, and that cause is traceable, perhaps, to the circumstance of the *Harvey Combe* being a six-wheeled coupled engine, whilst the other had four wheels uncoupled. Apart from all consideration

* Mr. Robert Stephenson's engine may have been equally powerful with the North Star, as, at 14 miles per hour, it dragged so heavy a load as to justify an opinion that its tractive force would equal the latter at $18\frac{1}{2}$ miles, but, for reasons given before, the evaporative data are not sufficiently correct to permit a safe deduction for consumption of power.

of the respective advantage, or disadvantage of these two methods of construction for tractive purposes, it is important, and not difficult to determine the respective consumption of power consequential to them.

I adduce these few comparisons to exhibit the facility and certainty with which they are developed by this method of investigation. Another instance of the delicacy of this test will be useful to shew the necessity of being informed of every circumstance which may influence the results of locomotive experiments. By referring to the two on the *Soho*, and comparing the momentum generated by 1 lb. of steam at the higher velocity, it appears that this engine realized a much less performance than any other on the list at the same speed; and, on the contrary, that its performance at the lower velocity greatly exceeded every other at that speed. Seeking for an explanation in Mr. Edward Woods's detailed report of those trials, the cause is distinctly made known. He notes, at the higher velocity, the engine then travelling from Manchester to Liverpool, "weather very wet, a strong head wind from the westward;" and on its trip from Liverpool to Manchester, same day, with a luggage train, "weather wet, wind westerly, but not so strong as in the previous experiment." In the one case, therefore, the engine was materially retarded, and in the other assisted by the wind. The augmented consumption of power at the highest velocity to produce equal effects, compared with other experiments, is shewn in Table XIV., for the velocities being as 1.40 to 1, and their squares as 1.97 to 1, the ratio of the momenta, or of the mechanical performance realized, comes out as 2.72 to 1, being an increase in the expenditure of steam at the highest velocity very much greater than that exhibited by any of the other comparisons at speeds bearing a similar ratio to each other. The fact, also, is evidenced by the result, that the engine was proportionally more retarded at the highest, than assisted by the wind, at the lowest velocity *.

The disturbance caused by the wind in these two experiments necessarily renders them unfit to be used for the general deductions exemplified in the four Means; and for instituting particular comparisons on the effects of engines, we are, hereby, forcibly warned of the necessity of using those results only which have been obtained under similar circumstances. It is possible that the experiments marked *erroneous* may have been affected by weather, and that the

* Part of the load was taken up the Whiston Incline by a bank engine, for which a deduction is made in computing the mean momentum at the lower velocity.

reported data were correct; but, if such were the case, they are equally unfit for comparison with engines not influenced by those causes.

This method of investigation discloses experimental defects, as well as errors of fact. Of the latter some instances have been noted, and having already, in explaining the objects of Table XIII., referred to the anomalous results of the *Fury*, we have only to turn to the comparative analysis in Table XIV. to be convinced of their inaccuracy, for we there find a less resistance to have been encountered at the higher of the two velocities, and, consequently, that a gain instead of loss of effect resulted. Similar anomalies would be manifested by reducing the other experiments marked *erroneous* to the terms of Table XIV., and by comparing No. II., on the *North Star*, with No. III.; also, No. V., with No. IV.; No. II. of the *Æolus* with No. III., &c.; but one example was sufficient for exposing this class of errors.

In conducting railway experiments—properly so called—it is important to trim the same engine and tender to the same weight in each trial, as any change in the amount of such weight, which is useless load, materially alters the product of useful tractive effect. It will be seen by comparing Experiments II. and V. on the *North Star*, that there is a difference in the weights of engine and tender of no less than 3.33 tons; and in the two experiments with the *Lion* of 3.43 tons. There is a notable mistake in the Æolus, Experiment IV., the difference being 18.42 tons between the gross and useful loads, instead of 28.41 tons, the weight of engine and tender, according to Mr. Wood, (p. 17, of his Report). Either the gross or useful load, therefore, is misstated, and that experiment ought to be excluded *.

The facts practically elicited in railway traffic, and generally well appreciated by the managers of railways, are the best possible indices of the economy resulting from so accommodating the load to the intended velocity, that the engine shall exert, at all times, as nearly as may be, its maximum force. The Tables present so few experiments with the same engine at like velocities, with unlike loads, that but few comparative results are afforded by them, but this view is corroborated by the *Neptune*, Experiments I. and II., at 23½ miles per hour, and is visible even with so small a difference between the useful loads as 5.65 tons; and to a proportionally greater degree by the *Lion*, in which two trials, at velocities nearly identical, the loads varied in weight 28.05 tons. It has been

* This is probably a clerical error, but was not discovered till after the Tables were printed.

before observed that these engines, dynamically considered, produced respectively an equal mechanical effect, with equal power, in the two cases; but, the useful effect, (Table XIII., column 8,) was smaller in each case, with the lighter load, the light load being saddled with the same useless weight of engine and tender as the heavier one, and equal momenta having to be generated by a like expenditure of power, to realize very unlike commercial effects. The skilful trimming of loads is, therefore, an object of no small economical consequence, as is apparent on the great scale of practice by comparing the cost of working merchandize and passenger trains; and it is still more evident by contrasting the locomotive expenses on well filled passenger trains on some railways, with the precarious amount of passenger traffic on others.

Under the head of experimental defects, detected by this method of analysis, may be classed those which arise out of referring positive facts and data to imaginary planes. In the *Leeds*, Experiment I., the engine actually travelled over $29\frac{1}{2}$ miles, at a mean velocity of 18.63 miles per hour, which multiplied into the mass moved, reduced to the terms of column 11, Table XIII. shews an absolute momentum acquired of 2606.60. M. de Pambour in his *new data* for the same engine, using the same experiment, assumes and applies for his ideal plane a velocity of 20.34 miles per hour, which, with the same mass, gives a momentum of 2846.27. He corrects the consumption of water by $\frac{1}{5}$th for waste, making its consumption 0.840 lb. per second; which being applied to both cases, the momentum generated by 1 lb. of steam per second is for the 1st, or real case, 3103.09, and for the 2d, or imaginary one, 3388.41. These sums prove the insufficiency of the method of reducing the real facts to the case of an ideal dead level; for, it is impossible that the same load should be moved at a greater velocity and realize a greater mechanical effect, than at a lower speed with equal power. It is to be observed too, that M. de Pambour originally considered the $29\frac{1}{2}$ miles as a level, in consequence of the help given up the incline by the bank engine; so that two different levels, giving necessarily different velocities, have been used by him for the same case; nor does the additional correction for water referred to before, (p. 101 of this paper,) set his *new data* to rights for the newly assumed level.

The same observations apply to the two cases of the *Hecla*, but from the care taken to ascertain, and apportion the consumption of water as steam in those cases, the error is not so great as in M. de Pambour's; still it is considerable, for the mechanical effects produced with equal power at the two velocities

are equal: but, we know that such a result must be inaccurate, and it arises from not applying a correction for the increased resistance encountered at a higher velocity, in the reduction of the acclivities and declivities to an imaginary level. The assumed dead level, as referred to time, may be theoretically correct, but it is clearly erroneous to superinduce that the same load could be moved over the same space in the shorter time, by the same expenditure of power actually required in the longer time; for, it is tantamount to saying that no additional resistance is opposed to an engine at high than at low velocities. The value of a theoretical rule is its sure, useful application to, and agreement with practical facts, an accordance which cannot obtain between the theoretical and practical results of the working of an ideal and actual railway, unless all the circumstances which affect the latter be taken into account. It appears from the experiments digested in these Tables that the sum total of resistance opposed to the steam, increases in about the ratio of the velocity of the engine. Should that fact be substantiated by subsequent, and more careful experiments—or whatever its amount may prove to be—it must be applied to the reduction of levels, or theory will not accord with practice.

The ascertainment of the ratio which the sum total of resistance opposed to the steam, at different velocities of an engine and train, bears to the ratio of those velocities—and whether such ratio be uniform, or variable, for similar differences at all comparative velocities—are problems which can only be solved by practical experiments on the power expended in the production of given effects. Hitherto, no measure of dynamic effect, appropriate to the characteristics of locomotive performance has been discovered, or proposed, to which the expenditure of power, or efficiency of the steam, could be referred, as decisive of the relative perfection of engines, or of the relatively constructive perfection of railways. It has escaped notice that the momentum generated by the power of a locomotive is the characteristic quality of its functions; and that it comprises all the mechanical effect of which any account can be taken, or from which any practical useful result can be drawn.

The economic properties of one engine are strictly comparable with those of another, by ascertaining the momentum which each can generate with equal power, at equal velocities. Upon this faculty of the engine depends the amount of useful tractive effect which can finally be realized.

That engine which, with the least power, generates the greatest momentum at a given speed, is evidently more economical of steam than another engine

producing a less momentum. It is the better engine, and all things being the same, it would drag a greater useful load with the same power, or attain a higher velocity with the same load.

The determination of the performance of locomotive engines by this method, is rendered as practicable, as exact, and as demonstrative of their relative powers and dynamic excellence, as the determination of the duty done by pumping engines; and the phrase *momentum* possesses the advantage, in common with the term *duty*, of being a true expression of a practical effect. The amount of momentum is derived from the mass, and from the velocity given to it, two facts which are more easily appreciated, and liable to less error than the measuring of pumps, lifts, &c., necessary for ascertaining the duty of pumping engines. The water consumed as steam being contained in vessels whose capacity is readily measurable, no difficulty exists in ascertaining the expenditure of power; and the computations for reducing, and comparing the results are of the simplest kind.

Though the work done by a locomotive may be reduced to the terms and expression of *horses' power*, that expression is, itself, a fallacy; and, until some instrumental means of accurately detecting the pressure *within* the cylinder, and on both sides of the piston, be made applicable to this engine, the determination of horses' power must rest on vague data obtained from *without* the cylinder, and can only be based on estimations of resistance which vary with every load, with every velocity, and which cannot be submitted to any positive test of truth.

Effects of a like nature can be obtained on all railways, and, with proper precautions in the conduct of the experiments, results of indisputable exactitude would be obtained, which could not fail to exhibit the precise, and comparative state of excellence of the engines and vehicles; for, not only the scientific effects, but the commercial results, dependent on the constructive perfection of engines and vehicles used on railways, are reducible to a common standard of comparison by this method.

The resistance, (inclusive of friction,) opposed to the progress of one engine and tender is measurable, in respect of another engine, by the power relatively consumed at equal velocities. In like manner, the special resistance of the load is ascertainable, and as that resistance arises, in great measure, out of the form, dimensions, and weights of the vehicles employed, and, as the final commercial results,—it may be said, even, the commercial prosperity—of railways depend,

in great measure, on the adaptation of the vehicles to the transport of the greatest number of passengers, or of the greatest weight of merchandise, for the least weight and bulk of matter set in motion, the importance of an exact method of comparing these respective qualities will be apparent; for, the forms and other properties of the vehicles on one railway, can be assimilated to those on another which may be proved to offer the least resistance to the motive power.

In the last three Tables referred to I have disregarded any comparisons of effect by the consumption of coke. No measure of power is derivable from the weight of fuel burnt, its effect having reference solely to its calorific strength, and to the qualities of the boiler. It is important to know the efficiency of the combustible, but the efficiency of the steam can alone determine the qualities of an engine. If the coke consumed by the engines of the Great Western, and London and Birmingham railways in Table XV., had been made the measure of effect, the results would have been 8 per cent. less in favour of the latter, as the coke consumed was in the ratio of 1 to 0.72, and the water as steam as 1 to 0.64, the Great Western being unity; but on a comparison between all the engines in the Tables, even 50 per cent. would not cover the difference in their evaporative results.

Table XV.

MEAN PRACTICAL RESULTS OF SIX EXPERIMENTS ON THE GREAT WESTERN, AND SIX EXPERIMENTS ON THE LONDON AND BIRMINGHAM RAILWAYS, AT EQUAL VELOCITIES.

	Velocity per hour.	Gross load.	Gross load moved by equal power, at the velocity.	Useful load.	Useful load moved by equal power, at the velocity.	Momentum generated by equal power, per second.	Weight of engines and tenders.	Width of gauge.
	Miles.	Tons.	Tons by 1 cubic ft. water.	Tons.	Tons by 1 cubic ft. water.	Mass × veloc.	Tons.	Ft. In.
GREAT WESTERN	27.02	89.11	0.677	62.33	0.4735	1545	26.78	7 00
LONDON AND BIRMINGHAM	26.90	67.79	0.801	51.00	0.6028	1821	16.79	4 8
	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.	Ratio.
GREAT WESTERN	1.000	1.00	1.00	1.00	1.00	1.00	1.00	1.00
LONDON AND BIRMINGHAM	0.995	0.76	1.18	0.81	1.27	1.18	0.62	0.66
	1	2	3	4	5	6	7	8

ENGINES AND VELOCITIES COMPARED.

GREAT WESTERN.

	Table XIII.	Miles per hour.
North Star	Experiment I.	18.63
Do.	IV.	31.52
Neptune	III.	29.77
Mean		26.64
Lion	I.	22.28
Apollo	II.	29.80
Venus	II.	30.16
Mean		27.41
Do. of the six Experiments		27.02

LONDON AND BIRMINGHAM.

	Table XIII.	Miles per hour.
Bury's Engine	Experiment I.	19.42
Do.	III.	31.29
Do.	II.	29.82
Mean		26.84
Harvey Combe	I.	21.85
Do.	II.	28.53
Do.	III.	30.51
Mean		26.96
Do. of the six Experiments		26.90

TABLE XIII.

RAILWAY.	NAME OF ENGINE.	No. of experiments.	Mean velocity of engine per hour.	Water as steam consumed per hour.	Gross load.	Gross load moved by one cubic foot of water as steam, at the velocity.	Useful load.	Useful load moved by one cubic foot of water as steam, at the velocity.	Mean velocity of engine per second.	Water as steam consumed per second.	Momentum generated per second.	Momentum generated per second by 1 lb. of water as steam.	
	Experiments by M. DE PAMBOUR.		Miles.	Cubic feet.	Tons.	Tons.	Tons.	Tons.	Feet.	lbs.	Mass × Veloc.	Mass × Veloc.	
LIVERPOOL AND MANCHESTER.	ATLAS	I.	9.72	43.56	206.90	4.749	190.00	4.3618	14.263	0.756	2951.01	3903.45	
		II.	15.00	48.30	139.54	2.889	122.64	2.5391	22.000	0.838	3069.88	3663.34	
		III.	18.15	46.51	52.05	1.119	35.15	0.7557	20.631	0.807	1439.40	1783.64	
		IV.	24.07	50.87	42.20	0.831	25.30	0.4973	35.302	0.883	1489.74	1687.13	
	VULCAN.	I.	26.90	57.92	47.41	0.818	34.07	0.5882	39.428	1.005	1869.28	1859.98	
	VESTA.	I.	31.60	60.53	41.86	0.691	28.15	0.4650	46.351	1.050	1940.25	1847.86	
	LEEDS	I.	18.63	60.51	95.41	1.576	83.84	1.3855	27.326	1.050	2606.60	2482.47	
		II.	26.70	65.86	44.08	0.669	32.01	0.4860	39.174	1.143	1726.78	1510.75	
	FURY.	I.	21.79	55.02	57.00	1.034	43.80	0.7960	31.957	0.955	1821.54	1906.78	Erroneous.
		II.	23.00	52.03	64.36	1.236	51.16	0.9832	33.733	0.903	2170.86	2404.05	
	LEEDS New data	I.	20.31	48.42	95.41	1.970	88.34	1.8244	29.832	0.840	2846.27	3388.41	
		II.	29.09	48.42	45.59	0.941	32.52	0.6716	42.66[illegible]	0.840	1945.09	2355.83	
	Experiments by MR. ROBT. STEPHENSON.	I.	14.00	77.00	220.00	2.856	204.75	2.6590	20.533	1.336	4583.26	3430.58	
		II.	35.00	77.00	40.00	0.520	24.75	0.3214	51.333	1.336	2053.32	1536.91	
GREAT WESTERN.	Experiments by MR. NICHOLAS WOOD. (Report on the Great Western Railway, 1838.) NORTH STAR. Diameter of cylinders . . . 16 inches Length of stroke . . . . . 16 inches Diameter of working wheels . . 7 feet	I.	18.63	141.4	194.70	1.376	166.36	1.1765	27.323	2.454	5319.78	2167.80	
		II.	25.30	153.6	79.26	0.516	53.58	0.3488	37.106	2.666	2941.02	1103.15	Erroneous
		III.	30.66	200.9	110.61	0.550	82.03	0.4083	44.967	3.487	4973.91	1426.41	
		IV.	31.52	163.8	61.66	0.376	32.92	0.2009	46.229	2.843	2850.48	1002.63	
		V.	33.90	136.5	70.62	0.517	41.61	0.3048	49.719	2.369	3511.05	1482.08	Erroneous.
		VI.	38.51	197.7	44.79	0.226	15.90	0.0804	56.481	3.426	2529.78	738.40	
	ÆOLUS. Diameter of cylinders . . . . 14 inches Length of stroke . . . . 16 inches Diameter of working wheels . 8 feet	I.	17.80	105.2	132.73	1.261	104.60	0.9943	26.106	1.826	3465.11	1897.65	
		II.	19.72	98.5	76.59	0.777	48.34	0.4907	28.922	1.706	2015.13	1181.20	Erroneous.
		III.	22.64	110.4	108.29	0.980	79.69	0.7218	33.205	1.916	3595.76	1876.70	
		IV.	23.83	114.7	82.23	0.716	63.81	0.5563	34.950	1.991	2873.93	1443.46	
		V.	27.90	129.5	78.83	0.608	50.46	0.3896	40.919	2.248	3225.64	1434.89	
		VI.	29.77	117.9	59.52	0.504	31.22	0.2648	43.662	2.046	2598.76	1270.16	
		VII.	33.27	130.9	52.75	0.403	24.27	0.1854	48.795	2.272	2573.93	1132.64	
	VENUS. Diameter of cylinders . . . 12 inches Length of stroke . . . . 16 inches Diameter of working wheels . 8 feet	I.	22.50	96.9	76.90	0.793	50.46	0.5207	32.999	1.682	2537.62	1508.51	
		II.	30.16	111.1	58.00	0.522	31.41	0.2827	44.234	1.928	2565.57	1330.69	
	NEPTUNE. Diameter of cylinders . . . . 12 inches Length of stroke . . . . . . 16 inches Diameter of working wheels . . 8 feet	I.	23.35	87.45	74.81	0.855	44.81	0.5121	34.246	1.518	2561.94	1687.71	
		II.	23.53	92.80	77.03	0.829	50.40	0.5437	34.480	1.611	2655.99	1648.66	
		III.	29.77	107.60	58.00	0.538	31.49	0.2920	43.662	1.868	2539.39	1355.66	
	APOLLO. Diameter of cylinders . . . 12 inches Length of stroke . . . 16 inches Diameter of working wheels . . 8 feet	I.	21.05	94.20	77.02	0.897	50.46	0.5355	30.873	1.635	2377.83	1441.75	
		II.	29.80	120.20	57.90	0.481	31.42	0.2613	43.706	2.086	2530.57	1213.12	
		III.	33.08	117.10	44.86	0.383	18.25	0.1558	48.517	2.032	2176.07	1070.08	
		IV.	34.70	129.50	35.93	0.277	9.37	0.0723	50.893	2.248	1828.58	831.14	
	PREMIER Diameter of cylinders . . . 14 inches Length of stroke . . . 15 inches Diameter of working wheels . 6 feet	I.	22.37	140.4	105.46	0.751	80.50	0.5733	32.809	2.437	3460.03	1419.79	
		II.	24.65	154.5	84.05	0.544	59.67	0.3862	36.153	2.682	3038.65	1132.98	
	LION Diameter of cylinders . . . 14 inches Length of stroke . . . . 15 inches Diameter of working wheels . . 7 feet	I.	22.28	145.6	104.10	0.717	80.50	0.5528	32.677	2.529	3411.47	1348.73	
		II.	22.95	110.3	76.78	0.696	52.45	0.4755	33.659	1.920	2584.33	1346.00	
LONDON AND BIRMINGHAM.	HARVEY COMBE. Diameter of cylinders . . . [illegible] inches Length of stroke . . . . 18 inches Diameter of working wheels . . 5 feet	I.	21.85	70.66	81.61	1.154	64.36	0.9150	32.046	1.226	2615.27	2133.17	
		II.	28.53	105.90	70.95	0.669	53.45	0.5047	41.843	1.838	2968.76	1615.21	
		III.	30.51	88.81	50.15	0.564	32.65	0.3676	44.717	1.540	2242.55	1455.54	
	BURY'S ENGINE. Diameter of cylinders . . 12 inches Length of stroke . . 18 inches Diameter of working wheels . 5 feet	I.	19.42	56.81	83.53	1.470	67.20	1.1828	28.482	0.986	2379.10	2412.88	
		II.	29.82	94.42	69.76	0.738	53.91	0.5703	43.735	1.639	3050.95	1861.41	
		III.	31.29	91.00	50.77	0.558	34.45	0.3785	45.891	1.579	2329.88	1475.54	
GRAND JUNCT.	HECLA. Experiments by DR LARDNER	I.	26.95	78.72	82.00	1.041	60.00	0.7621	39.433	1.366	3233.50	2367.13	
		II.	30.93	91.96	82.00	0.891	60.00	0.6524	45.323	1.596	3716.48	2328.62	
LIVERPOOL AND MANCHESTER.	SOHO. Experiments by MR ED. WOODS, 1839. Diameter of cylinders . . . 14 inches Length of stroke . . . 18 inches Diameter of working wheels . 5 ft. 6 in.	I.	21.96	82.28	145.42	1.767	133.92	1.627	32.210	1.428	4313.56	3020.70	
		II.	30.79	109.02	46.51	0.426	35.00	0.321	45.158	1.892	2099.75	1109.80	
	1	2	3	4	5	6	7	8	9	10	11	12	

Table XIV.

RAILWAY	NAME OF ENGINE.	Experiments compared from Table XIII	Respective actual velocities.	Ratios of the velocities.	Ratios of the squares of the velocities.	Ratios of the power expended to produce equal momenta.	Ratios of the power expended to produce equal gross effects.	Ratios of the power expended to produce equal useful effects.	Loss of momentum.	Loss of gross effect.	Loss of useful effect.
	RATIOS OF THE VELOCITIES OF THE TRAINS, and of the Power expended, at the respective Velocities, to produce equal Momenta, and equal Gross and Useful Effects.								LOSS OF MOMENTUM AND OF EFFECT, at the respective Velocities, by equal expenditure of Power		
			Miles per hour.						Per cent.	Per cent.	Per cent.
LIVERPOOL AND MANCHESTER.	ATLAS.	2 / 1	15.00 / 9.72	1.54 / 1.00	2.38 / 1.00	1.06 / 1.00	1.64 / 1.00	1.71 / 1.00	6.15	39.17	41.79
		4 / 3	24.07 / 18.15	1.32 / 1.00	1.75 / 1.00	1.05 / 1.00	1.34 / 1.00	1.51 / 1.00	5.42	25.74	34.20
	VESTA } VULCAN }	1 / 1	31.60 / 26.90	1.17 / 1.00	1.38 / 1.00	1.006 / 1.000	1.18 / 1.00	1.26 / 1.00	0.76	15.53	20.95
	LEEDS.	2 / 1	26.70 / 18.63	1.42 / 1.00	2.05 / 1.00	1.64 / 1.00	2.35 / 1.00	2.85 / 1.00	38.75	57.52	64.93
	FURY.	2 / 1	23.00 / 21.79	1.05 / 1.00	1.11 / 1.00	0.79 / 1.00	0.83 / 1.00	0.809 / 1.000	Gain. 31.00	Gain. 19.00	Gain. 23.00
	LEEDS. New Data	2 / 1	29.09 / 20.34	1.43 / 1.00	2.04 / 1.00	1.43 / 1.00	2.09 / 1.00	2.73 / 1.00	31.67	52.24	63.47
	STEPHENSON'S ENGINE.	2 / 1	35.00 / 14.00	2.50 / 1.00	6.70 / 1.00	2.23 / 1.00	5.49 / 1.00	8.28 / 1.00	55.20	81.80	87.93
GREAT WESTERN.	NORTH STAR.	6 / 1	38.51 / 18.63	2.06 / 1.00	4.27 / 1.00	2.93 / 1.00	6.03 / 1.00	14.63 / 1.00	65.94	83.58	93.17
		4 / 1	31.52 / 18.63	1.69 / 1.00	2.86 / 1.00	2.16 / 1.00	3.65 / 1.00	5.85 / 1.00	53.75	72.68	82.93
		2 / 1	25.30 / 18.63	1.35 / 1.00	1.84 / 1.00	1.96 / 1.00	2.66 / 1.00	3.37 / 1.00	49.12	62.50	70.36
	ÆOLUS.	7 / 1	33.27 / 17.80	1.86 / 1.00	3.49 / 1.00	1.67 / 1.00	3.12 / 1.00	5.30 / 1.00	50.32	68.05	81.36
		6 / 1	29.77 / 17.80	1.67 / 1.00	2.79 / 1.00	1.41 / 1.00	2.50 / 1.00	3.75 / 1.00	33.07	60.00	73.37
		5 / 1	27.90 / 17.80	1.56 / 1.00	2.45 / 1.00	1.32 / 1.00	2.07 / 1.00	2.75 / 1.00	24.39	51.79	60.82
		4 / 1	23.83 / 17.80	1.33 / 1.00	1.78 / 1.00	1.31 / 1.00	1.76 / 1.00	1.78 / 1.00	23.30	43.22	49.06
		3 / 1	22.64 / 17.80	1.27 / 1.00	1.61 / 1.00	1.01 / 1.00	1.28 / 1.00	1.37 / 1.00	1.20	22.29	27.41
	VENUS.	2 / 1	30.16 / 22.50	1.34 / 1.00	1.79 / 1.00	1.13 / 1.00	1.51 / 1.00	1.54 / 1.00	11.79	34.18	45.71
	NEPTUNE.	3 / 1	29.77 / 23.35	1.27 / 1.00	1.62 / 1.00	1.27 / 1.00	1.58 / 1.00	1.75 / 1.00	21.38	37.08	43.02
		3 / 2	29.77 / 23.53	1.26 / 1.00	1.60 / 1.00	1.24 / 1.00	1.54 / 1.00	1.84 / 1.00	19.51	36.99	46.30
	APOLLO.	4 / 1	34.70 / 21.05	1.64 / 1.00	2.71 / 1.00	1.73 / 1.00	3.23 / 1.00	7.40 / 1.00	42.33	69.12	86.50
		3 / 1	33.08 / 21.05	1.57 / 1.00	2.46 / 1.00	1.34 / 1.00	2.31 / 1.00	3.43 / 1.00	25.75	57.30	70.91
		2 / 1	29.80 / 21.05	1.41 / 1.00	2.00 / 1.00	1.18 / 1.00	1.86 / 1.00	2.04 / 1.00	15.83	46.38	51.21
	PREMIER.	4 / 2	34.70 / 29.80	1.16 / 1.00	1.35 / 1.00	1.45 / 1.00	1.73 / 1.00	3.61 / 1.00	31.49	42.42	72.34
		2 / 1	24.65 / 22.37	1.10 / 1.00	1.21 / 1.00	1.25 / 1.00	1.38 / 1.00	1.40 / 1.00	20.20	27.57	32.64
	LION.	2 / 1	22.95 / 22.28	1.03 / 1.00	1.06 / 1.00	1.002 / 1.000	1.17 / 1.00	1.16 / 1.00	0.21	2.93	13.99
LONDON AND BIRMINGHAM.	HARVEY COMBE.	3 / 1	30.51 / 21.85	1.39 / 1.00	1.94 / 1.00	1.46 / 1.00	2.04 / 1.00	2.48 / 1.00	31.77	51.99	59.83
		2 / 1	28.53 / 21.85	1.30 / 1.00	1.70 / 1.00	1.32 / 1.00	1.72 / 1.00	1.81 / 1.00	24.29	42.03	44.85
		3 / 2	30.51 / 28.53	1.06 / 1.00	1.14 / 1.00	1.10 / 1.00	1.18 / 1.00	1.37 / 1.00	9.89	15.70	27.17
	BURY'S ENGINE.	3 / 1	31.29 / 19.42	1.61 / 1.00	2.59 / 1.00	1.63 / 1.00	2.63 / 1.00	3.12 / 1.00	38.89	62.04	68.00
		2 / 1	29.82 / 19.42	1.53 / 1.00	2.35 / 1.00	1.29 / 1.00	2.00 / 1.00	2.06 / 1.00	22.86	49.98	51.79
		3 / 2	31.29 / 29.82	1.04 / 1.00	1.10 / 1.00	1.26 / 1.00	1.32 / 1.00	1.50 / 1.00	20.73	24.40	33.64
	SOHO.	2 / 1	21.96 / 30.79	1.40 / 1.00	1.97 / 1.00	2.72 / 1.00	4.14 / 1.00	5.50 / 1.00	63.26	75.90	80.28
	1	2	3	4	5	6	7	8	9	10	11

	MEANS.									
1.	Mean of 42 Experiments from Table XIII., being the means respectively of the 21 highest and 21 lowest velocities: the two experiments on the Fury, Nos. 2 and 5, of the North Star, No. 2 of the Æolus, and the Soho, omitted	30.92 / 20.32	1.52 / 1.00	2.33 / 1.00	1.43 / 1.00	2.43 / 1.00	3.11 / 1.00	30.11	58.90	68.06
2.	LIVERPOOL and MANCHESTER. Mean of the four first comparisons, the Fury omitted as erroneous	24.34 / 18.35	1.32 / 1.00	1.75 / 1.00	1.15 / 1.00	1.62 / 1.00	1.77 / 1.00	13.17	38.52	43.77
3.	GREAT WESTERN. Mean of all the compared Experiments	29.65 / 20.69	1.43 / 1.00	2.05 / 1.00	1.12 / 1.00	1.95 / 1.00	2.57 / 1.00	30.01	51.11	61.23
4	LONDON and BIRMINGHAM. Mean of all the compared Experiments	30.32 / 23.48	1.29 / 1.00	1.66 / 1.00	1.34 / 1.00	1.79 / 1.00	2.05 / 1.00	25.70	44.26	51.22

OF THE BLAST, AND THE RESISTANCE OCCASIONED BY IT.

The intensity of the pressure subsisting on the opposite side of the piston from the blast has been but imperfectly illustrated by writers on the locomotive engine. The discharge of the steam has been likened to a jet, and that jet considered as continuous, which involves a contradiction, or, at best, a confusion of terms. By the passages subjoined it is clearly M. de Pambour's opinion that the evacuation of a cylinder full of steam has a duration equal to that of a single stroke of the piston; for, to an engine having a blast-pipe orifice $\frac{1}{38}$th of the area of both cylinders, he assigns the velocity at which the steam escapes to be 38 times greater than that of the pistons. We have here an erroneous observation of the phenomena of the blast, and an erroneous determination of its velocity *.

Every observer can appreciate by his ear that an interval exists between the alternate discharges of steam from the two cylinders. The interval between three discharges marks the time which elapses between two successive discharges from the same cylinder; thus, the period between two discharges, from the same cylinder, comprises the time of one discharge, and of two pauses. The velocity of escape is, therefore, as much swifter than that assigned to it by the author, as the difference between the duration of a single jet, and the period intervening between the cessation of one, and the commencement of another jet from the same cylinder. That these jets are periodic, and not continuous, is distinctly evidenced by the audible pulsations in the chimney, even at the very

* "With an orifice $2\frac{1}{2}$ inches in diameter, or 5 square inches area, and cylinders of 11 inches diameter, or 190 square inches total area; that is to say, with an orifice which is only $\frac{1}{38}$th of the area of the cylinders, we see, that in order that all the steam may get out by that passage, its speed in passing through the orifice must be 38 times as great as it was in the cylinder.

"The velocity of the jet formed in the chimney will then be, for the dimension we consider, equal to 38 times the velocity of the piston, or in other words, equal to $6\frac{1}{2}$ times the speed of the engine, the latter speed being nearly six times as great as that of the piston.

"Thus the power of this additional means will be greater in proportion as the velocity of the engine itself will be more considerable," &c.,—"and as that velocity cannot be produced merely by the tendency of the steam to escape into the atmosphere, a part of the power of the engine itself must necessarily in those great speeds, be spent in expulsing the steam; that is to say, in blowing the fire in the fire place," &c.—(*Treatise on Locomotives*, page 247.)

highest velocities of an engine, and their duration may be measured at the lower speeds. Now, supposing—what is, probably, the fact—that, at some certain velocity of the piston, the duration of one complete evacuation, or cylinder full, corresponds with the period of the interval elapsing between it and the next discharge from the same cylinder, it is evident that the velocity of the jet, in that case, would be exactly twice as great as the velocity assigned to it by M. de Pambour's theory.

It is important to have a clear perception of the intermittent action of the blast; for, upon that action depend, in great measure, the resulting pressure against the piston, and the production of a sufficient current of air through the fire, both which effects would be materially changed in intensity, by the substitution of a continuous for a periodic current.

On opening the eduction valve the compressed steam in the cylinder exerts, for an instant, its maximum pressure against the piston, but, by virtue of its elastic force it expands with immense rapidity, and quits the cylinder at a velocity as much greater than that of the motion of the piston, as would be expressed by multiplying the difference between the areas of the cylinder, and of the blast-pipe orifice, into the difference between the time occupied in the escape of the steam, and the duration of a stroke of the piston. Assuming, for an example, that a single discharge is effected in $\frac{1}{4}$th of the time of a single stroke, the velocity of the jet into the chimney would be 19×4 (for the dimension of cylinder and blast orifice cited by M. de Pambour,) or 76 times as great as the velocity of the piston, but the true ratio between them cannot be known, until the exact duration of a jet be determined for different speeds of the piston, and for different acting pressures of steam. It will be observed that the velocity of the jet is here considered as that from a single cylinder, not from both cylinders as by M. de Pambour. Half the steam ejected from the cylinders on the above supposition of the periods of blast and of pause, gives twice the speed assigned by the author, and as the jets from the two cylinders alternate, the real velocity of the blast must be calculated on that from a single cylinder.

Although experiments are requisite for determining the precise duration of the jet, or the time occupied by the steam in evacuating the cylinder, its period is ascertainable within a definite limit.

The ear and the eye distinguish the lapse of a sensible interval of time between every two discharges, and these discharges proceed from two cylinders. The two engines—for the locomotive carries two—are so combined that a dis-

charge from the one commences at the half stroke of the piston of the other, or at periods definitively marked by spaces of 90 degrees on the revolution of the crank shaft, during which, four complete blasts occur. The consequence is that we know a single discharge to be completed within the time occupied by the piston in accomplishing a half stroke, or before the piston has traversed half the length of the cylinder; yet, the discharges are audibly distinct, and the pause between one discharge and its successor is perceptible. This pause may equal, or it may be longer or shorter than the duration of the jet, but we see that a single blast cannot occupy the fourth part of the time of the revolution of the crank shaft, and that, very probably, it does not exceed the eighth part, or the period of a quarter stroke of the piston. It must be also borne in mind that the same principle holds good at all velocities of the engine.

Under no circumstances, therefore, does the pressure from the blast oppose the piston much longer than during the fourth portion of its course; its intensity is at a maximum at the instant of the commencement of a return stroke, at which time the piston is at its slowest speed; and its minimum occurs at the instant of the completion of the discharge: the mean of these two pressures—which cannot exceed the half of that of the active steam—operates, therefore, as a retarding force during a period of less than that of the half stroke; and, probably, it never exceeds in pressure the fourth of the intensity of the active steam, reckoned upon the whole stroke, with the usual dimensions of the blast-pipe orifice. Upon this supposition, and with an active pressure of 30 lbs. per square inch, the mean resistance from the blast would not be greater than $7\frac{1}{2}$ lbs.; and, with half that active pressure, or 15 lbs., it would not exceed $3\frac{3}{4}$ lbs. per square inch against the pistons.

Before proceeding to cite some observations and experiments which will tend to corroborate this argument, I may conclude the remarks upon the nature of the blast in urging the fire, the process for which alone it is intended. The evacuation of the contents of the chimney, by the periodic blasts of steam through it, effects this object; a continuous discharge of steam into the chimney would defeat it, as the chimney could not be occupied by steam, and by the products of combustion at the same time. A succession of discharges, and a succession of intervals, are, therefore, essential to the success of the process, for, from the expansive and condensible nature of steam, a temporary and partial vacuum is formed in the chimney on the cessation of a discharge, which is almost instantaneously replenished by the pressure of the air into the fire, and

through the boiler; another discharge takes place, the chimney is again evacuated of its contents, and so on. Thus, the jets of air into the fire alternate with the jets of steam into the chimney.

It is practically found that, at high velocities of the engine, evaporation is accelerated; consequently the fire is urged to more intense action, and the combustion of the fuel is more rapid in a given time. This result is the natural effect of velocity in the engine; for, the number of discharges of steam into the chimney is proportional to the speed of the piston; the number, therefore, of jets of air into the fire is augmented in like proportion: perhaps, also, the duration of the jet into the fire is longer, with respect to that of the jet of steam into the chimney, at high than at low velocities; for, though a cylinder full, or an equal measure, composes the volume of the jet of steam at all velocities, its density varies with the speed of the engine, (under the ordinary circumstances of railway traffic,) and the rate of expansion, or relative duration of a discharge may be influenced by it. The less dense steam may, therefore, occupy less time in escaping from the cylinder, than the more dense steam. Supposing such to be the case, the frequency of the discharges would not only be increased in the ratio of the velocity of the piston, but the interval between the discharges, or the time given for air to enter the fire, would be also increased; whence, the increased evaporative power of the boiler.

It is the frequency of the jets into the chimney which enables a locomotive boiler to vaporize as much water in a given time at a high, as at a low velocity; but, in order that it should vaporize more water in equal times, some additional cause seems to be required, for more air must be admitted to the fire, inasmuch as the combustion of the fuel must be swifter. We must, therefore, suppose either the vacuum left in the chimney at the higher velocity to be somewhat greater than at a lower speed, or the discharge to be more swiftly effected, so as to increase the relative length of the intervals between the discharges. The fact that an equal evaporation can be maintained at high, as at low speeds, shews the jet of lower steam to be equally as effective as the jet of higher steam; and this fact is consistent with the office of the jet, which being little more than that of emptying the chimney of its contents, a cylinder full of steam at 15 lbs., or much less pressure, would be as efficient as the same measure at 50 lbs. per square inch.

The engine under my charge (previously referred to p. 78, and upon the boiler of which, described p. 71, the experiments were made on the

constituent heat of steam) furnished a favourable opportunity for investigating the effect of the blast in increasing resistance. The cylinders were 10 inches diameter, stroke 24 inches, velocity of piston 240 feet, or 60 double strokes per minute. The cylinders being fixed horizontally on each side of the fire-box of the boiler, and the blast-pipes joined to their under sides, the latter were not less than 10 feet long from their junction with the cylinders to their termination in the chimney. These pipes united at the smoke box, were 3 inches diameter, and the blast orifice was $2\frac{1}{4}$ inches. A fly wheel 7 feet diameter, weighing 14 cwt., and having 65 revolutions per minute, was driven by the crank shaft, the movement of the machine which carried the engines, and to which they gave locomotion, being too slow (viz., 5 feet per minute) to enable the cranks to pass the centres smoothly without additional momentum.

I was able to assign certain loads to the engines which, at the above velocity, severally required $1\frac{1}{2}$, 4, 8, 15 and 20 lbs. pressure per square inch on the pistons to overcome them. The friction of the engines, unloaded, was balanced by $1\frac{1}{2}$ lb. of steam; they stopped with 1 lb. This, like all the other piston pressures here spoken of, is to be considered as denoted by that in the boiler, maintained, on experimental occasions, at the degree which would just balance the load, at the common velocity of 60 strokes per minute. It is probable, at this moderate speed of the pistons, that the elasticity of the steam in the cylinders coincided very nearly with that in the boiler. It has been before stated that a thermometric steam-gauge was attached to the boiler, which gave sure indications of the pressure within it.

The immediate cause of my entering on these experiments is worth mentioning. I one day observed the mechanic in care of the machine, whilst preparing for work, opening and shutting the grease cocks of a cylinder, and giving oil to the piston. The engines were then working without load, and it was evident that a small vacuum existed, after the blast, or the oil would have been blown back instead of entering the cylinder. This fact, the possibility of which had not before struck me, induced me forthwith to order another gauge from Mr. Adie, which was fixed on one of the blast-pipes, in a convenient place for constant observation, about $2\frac{1}{2}$ feet from its junction with the cylinder, the bulb being exposed to the full current of the escaping steam. This instrument detected the fact of a vacuum by marking, usually, a temperature of from 208° to 210°, or about 1 lb. per square inch below the atmospheric pressure, the active steam on the piston being $1\frac{1}{2}$ lb. above it. When the engine was driven at

double velocity, or at 120 revolutions per minute, at which speed it required about $3\frac{1}{2}$ lbs. of steam, the thermometer rose to 211°, and when locomotion was given to the machine at the usual velocity of 60 revolutions of the crank shaft, and requiring 4 lbs. in the boiler, the blast thermometer stood at 212°, exhibiting a pressure equal to the atmosphere only. At 8 lbs. on the piston, a counter pressure of about 2 lbs. was exhibited, at 15 lbs., about 4 lbs., and at 20 lbs. the blast thermometer indicated 6 lbs., beyond which point I was unable to load the engines.

At all these pressures a phenomenon was visible which was to be expected, viz., small and continual oscillations of the mercury in the thermometer, for it is clear that, at the rapidity with which the discharges succeed each other, sufficient time is not allowed for the mercury to assume either the maximum or minimum degree of temperature of any one blast. Nothing could be gathered from a syphon gauge applied to the pipe at the same point with the thermometer, as the mercury was kept in a state of incessant and violent oscillation. I may also notice another fact well known before, but well shewn by the blast thermometer, viz., that, at whatever pressure the steam existed in the boiler, the blast-thermometer always marked the same degree for the same load and velocity, proving thereby that steam invariably assumes in the cylinder of an engine the pressure due to the load. Under all the loads cited I have worked the engines at their regular velocity, with steam in the boiler from 50 lbs. downwards, and the blast thermometer always gave the same indications as when the steam in the boiler was only of just sufficient force to overcome the load. I have also repeatedly worked the engines alone, and under each of the above loads, with greatly varying velocities, and the blast thermometer instantly rose with an increase of pressure in the cylinders, and as instantly fell by partially closing the regulator, thereby reducing the pressure, and with it the velocity of the pistons. It is, thus, manifest that the counter resistance from the blast augments and diminishes with the pressure on the pistons, and that it is independent of the velocity of the piston; for, in all these observations it only rose with the velocity, when the pressure was also increased.

It has been assumed by M. de Pambour, Mr. Wood, and Mr. Robert Stephenson, it is also, I believe, a generally received doctrine, that the pressure from the blast increases with the velocity of the railway locomotive; the active pressure on the piston being presumed to be less at high, than at low speeds. No experiments are stated by any one of these authors to justify their

hypothesis; and an inference, as to blast pressure, founded on the increased evaporative power of the boiler at the higher velocities of the engine, cannot be admitted as consequent; for, increased evaporation simply denotes an effect arising from more frequent discharges into the chimney, an effect which proves nothing as to pressure against the piston. The fact of a greater resistance accompanying the discharge of low, than of high steam, cannot be admitted without proof obtained from demonstrative experiments; for, it seems to be contrary to the nature of steam, and is unsupported by a single argument, or a single experiment.

The "some hundreds of experiments" referred to by M. de Pambour, (pp. 90 and 91, *New Theory of the Steam Engine,*) and which he says, "will be found related in the second edition of our *Treatise on Locomotives*"—but which second edition has not yet been published—were made "by the application of a thermometer, and of an air-gauge or manometer to the pipe through which the steam, after having terminated its action in the engine escaped into the atmosphere." In this respect they resemble the experiments above related, but in the pages cited which contain this passage, the author only states his conclusions that the temperature of the escaping steam truly indicates its pressure, in which we are agreed. He has left us, for the present, in the dark as to the data, and reasoning on which he grounds his assertion (p. 161, *same Work,*) that the resistance caused by the blast increases in the direct ratio of the velocity of the piston. I may here, also, observe that he has left us equally at fault for the data, and experiments on which he grounds his conclusion that the resistance to a train, from the air, increases as the square of the velocity.

The foregoing experiments on the blast were commenced in the autumn of 1836; they were witnessed by many engineers and mechanics; in fact, the two thermometric gauges formed part of the apparatus of the engines; every time they were worked, observations of the thermometers took place, and an experiment may be said to have been daily made; but I have yet to relate two others, of a still more satisfactory nature, on the same engines.

Reflecting on its indications, it struck me that the blast thermometer could not very accurately determine either the maximum or minimum temperature of the steam in the pipe; and that, with still less certainty, could it indicate the mean pressure of the steam, during an entire stroke in the cylinder; for the pipe, with respect to a cylinder, can only be considered as a cock with respect to a boiler. So great is the well known rapidity of expansion, that steam

of a higher pressure, in passing from a denser to a rarer medium, as from a boiler into the atmosphere, instantly falls considerably below the temperature of the boiling point. This is a condition of the issuing steam, but the elasticity within the steam receptacle, whether it be a boiler or a cylinder, cannot be accurately ascertained by the temperature or pressure of the steam without the receptacle—however large the orifice of exit through a pipe communicating with the atmosphere—for the diminishing pressure within the cylinder must always have a relation to the capacity of the cylinder, and to the rapidity of the evacuation. It must also be observed, that the piston of an engine is in motion during the discharge, and that its effect must be, whilst assisting—for however short a time—in the expulsion of the steam, to compress it in some degree. Neither a thermometer, nor any other instrument placed without the cylinder seemed to me, for these reasons, to possess the faculty of truly indicating the pressure of the steam, at any one instant, within the cylinder. I considered the thermometer to have marked, within certain limits, the pressure in the blast-pipe, but not in the cylinder, and as I could not apply an indicator to the latter, I determined on employing another method of arriving at the exact pressure occasioned by the blast. This method was the synthetical one of working the same loads, at the same velocity, both with and without the blast, which would give certain results, and free the question from all possible error, and all doubt. An opportunity occurred for performing these experiments in November 1837; they were tried on loads requiring, with the blast, 20 and 15 lbs. pressure per square inch on the piston, respectively, in the following manner:—

Preparation was made for removing the blast-pipes at an instant's notice, by loosening the flaunch screws, as I well knew an experiment could not be long continued without blast, and with peat for fuel, which I was using at the time. The pressure in the boiler being brought accurately to 20 lbs., and the engines working at their proper velocity, the pipes were released by attendants; the engines, as might be expected, were for an instant accelerated, but with the regulator in my hand, were immediately restrained; the steam soon began to fall in the boiler, the throttle valve was opened in proportion, and when fully opened, the engines making 60 strokes per minute, the thermometric steam-gauge on the boiler exhibited 16 lbs., instead of 20 lbs. required with the blast: 4 lbs. pressure per square inch was, therefore, the true resistance of the blast, instead of 6 lbs. as previously indicated by the blast-pipe thermo-

meter. The engines were allowed to work till the load pulled them up, when the pressure in the boiler was at 14 lbs.

The steam was again raised, and the experiment repeated with similar results.

The load was then changed for that requiring 15 lbs. with the blast, and I commenced the experiment at that pressure, with which the engines freely started their load. The thermometer marked 12 lbs. as the elasticity of the steam when it had unrestrained admission to the cylinders, and the engines their stated velocity. The mean resistance of the blast at this load was, therefore, 3 lbs., instead of 4 lbs. indicated by the blast-pipe thermometer. The engines were pulled up at $10\frac{1}{2}$ lbs.; a repetition of the experiment produced precisely similar results.

The blast-pipes were then replaced, the above loads severally laid on, and the pressures in the boiler and blast-pipe ascertained to be the same as previously indicated.

I have regretted that I did not seize the same opportunity of trying the effects, without blast, at still lower pressures, as I have not since been able to renew them; but they are comparatively unimportant, particularly as respects their bearing on the railway locomotive; the operation, too, was not a little fatiguing and unpleasant, from my being half smothered by the steam and spray which escaped from the cylinders.

It was thus determined, by conclusive experiments, that $\frac{1}{5}$th of the power of the engine was absorbed, at a working pressure of 20 lbs. upon the inch, to blow the fire; and the same at 15 lbs. The length of stroke being greater in my engine than in the ordinary locomotive, would give a result at all pressures, probably somewhat lower for blast resistance than at shorter strokes, but the length of pipe would, perhaps, be against them. These experiments require, however, to be much more varied before it can be safely concluded that the same engine loses power from the blast, in the ratio of the pressure upon the piston, which is the result of the experiments cited. The indications of the blast-pipe thermometer seem to me to have denoted that a less proportional resistance occurs at very low, than at very high pressures.

The ingenious mechanic cannot fail to perceive, from the illustrations here presented of the phenomena of the blast, as they affect both resistance and combustion, that considerable economic improvement may be made in its action;

X 2

it is a species of *blower* which has the merit of simplicity, but it is a very costly appendage to a locomotive engine.

Recurring to the velocity of the blast at its issue into the chimney, I am able to assign its speed, very nearly, for the engines on which these experiments were made. It has been already shewn to be a condition, that the duration of a single jet cannot exceed that of a half stroke of the engine. A double stroke was effected each second, consequently a half stroke was completed in the fourth part of a second. A small hole was perforated at the lowest point of the under side of the blast-pipes, to allow the condensed water to escape, which formed in considerable quantities in the cylinders and pipes at first starting the engines, and from the following cause. The blast-pipes, on entering the smoke box, passed through 2 feet of water, below the cylindrical part of the boiler, which was always kept full, to cool by its evaporation the part of the smoke box exposed to the current of heated air from the 5 boiler tubes previously described. These holes were near the cylinders, and could be opened and closed at will; a small jet was given out by them, of equal duration with that into the chimney, which served me as a test of the latter, as follows:—Holding a fine steel marker against the gland of the slide valve, I could observe the commencement and termination of the little jet, and mark their instants on the spindle as it traversed, without removing the eye; and, the motion of the spindle being the counterpart of the piston, the duration of the blast was manifested by the marks, on measuring the portion occupied on the entire stroke of this valve spindle. Repeated observations of this kind demonstrated that the discharge was effected in about $\frac{1}{4}$th of the period of a single stroke, or in about $\frac{1}{8}$th of a second; there was, consequently, an interval of $\frac{1}{8}$th of a second between the termination of a discharge from one cylinder, and the commencement of a discharge from the other cylinder, as the eduction valve of the latter would be opened at the half stroke of the former, which half stroke, at the velocity of 60 revolutions per minute, was performed in $\frac{1}{4}$th of a second.

The escape of the steam from the cylinder was, thus, 4 times swifter than the motion of the piston; the areas of the cylinder and blast orifice were to each other as 19.783 to 1, expressing also the ratio of the relative velocity of the steam out of the cylinder, and into the chimney. The steam, therefore, issued from the blast orifice 79.13 times faster than the piston moved in the same time; and the piston travelled 6 inches in $\frac{1}{8}$th of a second; data which

will be found to give $316\frac{1}{2}$ feet per second, or 215 miles per hour as the rate of velocity of a single jet of steam into the chimney.

This determination of the velocity of the blast jet is a mere statistical fact from particular dimensions of engine, working at a given speed. High numbers always astonish, and they are often cited, whether useful or not; but as incorrect methods had been employed to ascertain similar data, and deductions from such data had been made the basis of fallacious reasoning, both as regards the velocity of the current of heat into the chimney, and blast-pressure, no apology is necessary for my stating the foregoing facts. To render them useful, and connect them with the action of the currents of air into the fire, and into the chimney, another series of experiments, and a different method of investigation would be required.

OF THE EXPENDITURE OF POWER FOR A GIVEN EFFECT BY FIXED AND LOCOMOTIVE NON-CONDENSING ENGINES.

But few direct and conclusive experiments appear to have been made on the expenditure of steam, for a given effect, by non-condensing engines. Of the two in Table VI., Experiment III. is the most satisfactory, as I had opportunities of submitting that engine to very frequent trials; nevertheless, the method employed of ascertaining the pressure was not so perfect as might be wished, as I had no indicator. The absolute pressure on the piston, with the engine loaded, was 14 lbs. per square inch*; its friction, unloaded, was overcome by 4.45 lbs., leaving 9.55 lbs. as the effective pressure.

By comparing the volumes of steam and water consumed per minute, by this engine, we have the ratio of 749 to 1, giving the pressure corresponding with it in M. de Pambour's table, as 21.13 lbs. above the atmosphere. This ratio should have been 771 to 1, for perfect agreement between the elasticity deduced from these comparative volumes, and that denoted by the instrument employed; an identity which would be obtained by augmenting the volume of steam by $\frac{1}{40}$th for the ineffective quantity contained in the passages, &c., an amount not considered, as I could not measure it. A nearer approximation to perfect agreement between the indications of the test per volume, and the real consumption of an engine can rarely be made.

* Throughout this paper, the term *absolute pressure* is used in contradistinction to *effective pressure*, and exclusive of the value of the atmosphere, or 14.71 lbs. per square inch, except when specially mentioned.

The absolute power of this engine was 29.32 horses, and its effective force 20 horses, which were respectively accomplished by the consumption of 81.85, and 120 lbs. of water as steam per horse-power per hour.

It is usually found that $\frac{1}{5}$th of the total power is absorbed by the friction of a good condensing engine; a proportion which would shew the Albion Mills engine (Experiment V. Table VI.) to have exerted an absolute force of $62\frac{1}{2}$ horses, obtained by the expenditure of 55.60 lbs. of water as steam per horse per hour; its effective power being 50 horses, and the consumption of water 70 lbs. per horse per hour.

The ratio of expenditure of power, for equal useful effects, between these two engines—so loaded—was, therefore, as 70 to 120, or as 1 to 1.71.

In the Appendix to a highly ingenious and philosophical "Report on Railways" made by my late esteemed friend, Mr. Charles Sylvester, of Derby, and addressed to Charles Lawrence, Esq., of Liverpool, in 1824, will be found a learned disquisition on the relative consumption of power by the above two classes of engine. Mr. Sylvester's knowledge and accurate theoretical analysis of the subject, are shewn by the close accordance of his conclusions with the facts established on these two engines, at their respective working pressures, viz. "that the relative economy of the condensing, and non-condensing engines, will be as the quantities of steam consumed, or as 1 to 1.72." These deductions were drawn from calculations on a condensing engine supposed to be loaded to Watt's standard—similar to the Albion Mills engine—and on a non-condensing engine supposed to be working with an absolute pressure of 15 lbs., or an effective load requiring 10 lbs. per square inch to overcome it, being half a pound more than the one above cited.

Mr. Sylvester then shews that by increasing the pressure upon the same non-condensing, and by enlarging the area of the condensing engine's cylinder and air-pump, so as to maintain the steam in it at an uniform pressure per square inch for all loads, the economy of the former would gradually approach, and finally equal that of the latter; thus shewing the consumption of steam by the non-condensing engine, for equal effects, to diminish with the pressure on its piston. This is, also, an irrefragable argument as will presently be seen, and was employed by the author in advocating the application to the locomotive of a species of atmospheric cooler, or condenser without water, which would, he thought, bring it more quickly on a par with the condensing engine, as respects economy of steam.

Since the period of Mr. Sylvester's investigation, the locomotive engine has assumed definite characteristics; and, to the resistance of the atmosphere, is superadded that arising from the blast-pressure, which has, hitherto, considerably enhanced the difficulty of determining both its net power, and its consumption of steam, for a given effect.

In the numerous comparisons contained in the foregoing pages between the locomotive, and fixed non-condensing engines, the consumption of the latter has been used, together with that of the condensing engine, as the test of the accuracy of the data of resistance assigned to the former by the various analysts; but, accurate determinations of the expenditure of steam by the same locomotive engine on which the values of friction, and blast-pressure were ascertained, as given in the preceding section, enable me to fix its consumption of water as steam for given effects, and thus to narrow the ground of doubt, and to establish still more correct data for ascertaining the real resistance opposed to progressive motion on railways, than such previous comparisons could furnish.

It was demonstrated, in the manner shewn in the last section, that, with the respective absolute pressures of 15 and 20 lbs. per square inch upon the piston, the resistances from the blast were 3 and 4 lbs. per square inch against the piston. I had frequently ascertained the weight of water as steam consumed by the engine when giving locomotion to the mass on which it was planted, with a pressure of 4, and 8 lbs.; and, on completing the experiments with, and without the blast-pipe, I ascertained the expenditure of water as steam, for several hours, with the load requiring 15 lbs. per square inch; this took place steadily at the rate of 1220 lbs. per hour. At that pressure, the absolute power of the engine was 17.13 horses, found as follows:

$$\overset{\substack{\text{Area of both}\\ \text{pistons.}\\ \text{Sq. in.}}}{157.08} \times \left(\frac{\overset{\substack{\text{Pressure.}\\ \text{lbs.}}}{15} \times \overset{\substack{\text{Motion of piston}\\ \text{per min.}\\ \text{feet.}}}{240}}{33000\text{ lbs.}}\right) = \overset{\text{Horse-power.}}{17.13}.$$

The water consumed per horse per hour for the absolute power was, consequently, 71.22 lbs.

The effective power balancing the load is thus found:

$$\overset{\text{lbs.}}{15} - \left(\overset{\substack{\text{For friction.}\\ \text{lbs.}}}{2.50} + \overset{\substack{\text{For blast}\\ \text{pressure.}\\ \text{lbs.}}}{3}\right) = \overset{\text{lbs.}}{9.50} \text{ per square inch; and}$$

$$\overset{\text{Sq. in.}}{157.08} \times \left(\frac{\overset{\text{lbs.}}{9.50} \times \overset{\text{feet.}}{240}}{33000\text{ lbs.}}\right) = \overset{\text{Horse-power.}}{10.84}.$$

The water consumed per horse per hour for the effective power was, therefore, 112.54 lbs.

It will be observed that I take the friction of the engines at $2\frac{1}{2}$ lbs. per square inch, though they worked, without load, with a pressure of $1\frac{1}{2}$ lb. of steam; but, it must be borne in mind there was a *vacuum* equivalent to 1 lb. on the opposite side of the pistons, which *vacuum* disappeared at 4 lbs. per square inch. The friction, therefore, must be valued as consuming steam of $2\frac{1}{2}$ lbs. per square inch for all pressures upon the piston exceeding 4 lbs.

I will now bring the absolute pressure of 15 lbs., as exhibited by the thermometric steam-gauge on the boiler, to the test of the pressure resulting from the comparative volumes of steam and water expended.

Area of both cylinders $\overset{\text{sq. in.}}{157.08} \times \overset{\text{in.}}{48.25}$ length of double stroke and spaces, $+\overset{\text{cub. in.}}{576}$ contents of passages, $-\overset{\text{cub. in.}}{301.59}$ contents of piston rods per double stroke, $=\overset{\text{cub. in.}}{7892.79}$; which, divided by $\overset{\text{cub. in.}}{1728}$, gives $\overset{\text{cub. ft.}}{4.567}$ as the volume of steam consumed per double stroke; and this sum, multiplied by 60 strokes, gives 274.02 cub. ft. as the volume of steam expended per minute.

The weight of water being, as aforesaid, 1220 lbs. per hour, the volume consumed per minute was 0.325 cub. ft.

The ratio of the two quantities is 843 to 1, equal, by M. de Pambour's Table, to a total pressure of 31.50 lbs.; from which deducting 14.71 lbs. for the atmosphere, (of which no account is taken for the force subsisting on the opposite side of the piston,) we have 16.79 lbs. per square inch, for the absolute pressure on the pistons, instead of 15 lbs. as indicated by the thermometer.

The quantities found for the pressure and water are, however, positive; the volume of steam is, also, positive; but, some of the steam was unavoidably condensed from the exposed state of the cylinders and steam-pipes; and, to bring the pressure deduced from the volumes to perfect identity with that marked by the thermometer, the volume of water requires to be diminished from 0.325, to 0.308 cub. ft. per minute, or by $\frac{1}{19}$th; which difference—on the supposition that the volume test is perfect, at the ratio of 887 to 1, and that the pressure in the cylinder was truly marked by that in the boiler—would be the precise value of the condensation.

We have thus a very near agreement between the pressure on the pistons derived from a test by a thermometer, which accorded with the safety valve, and a test derived from the comparative volumes of steam and water expended.

The consumption of steam per effective horse-power per hour has been

shewn to be 120 lbs. for the fixed non-condensing engine, (Exp. III., Table VI.,) and for the locomotive under review 112.54 lbs., which proves the latter to have been the most economical of the two, at nearly the same absolute pressures; the former being 14, and the latter 15 lbs. per square inch. The friction of the former was 4.45 lbs., and friction and blast resistance together amounted to 5.50 lbs. in the latter. This is a new and, perhaps, an unexpected result; it shews the locomotive from its smaller cylinders, smaller mass in motion, lighter and smaller rubbing surfaces, to be encumbered with little more than half the frictional load arising from the larger cylinder, heavy beam and connecting rod, and more massive apparatus of the fixed engine. It is proper, however, to observe that an economy of the power expended in friction is dearly bought by the locomotive at the expense of durability; for no short-stroke engines will endure like the longer ones, as is strikingly exemplified by the brief existence of the locomotive in a sound and healthy state, compared with the longevity of a house-engine.

Having now found the expenditure of steam, for a given effect, by a locomotive engine working at a pressure of 15 lbs. per square inch, I will apply the data to other pressures, and examine the results; and, first, on the load requiring 20 lbs., at which pressure the resistance for blast was ascertained, on the same engine, to be 4 lbs. on the opposite side of the piston. The nature of this load did not permit a lengthened account of the water to be taken, but there is nothing more certain than that the consumption of steam must be proportional to the resistance overcome; and that if with an absolute pressure of 15 lbs. the engine consumed 1220 lbs. of water per hour, 1425.31 lbs. would be required by the same engine, in the same time, moving at the same velocity, with a pressure of 20 lbs. per square inch. The absolute power of the engine, at this pressure, was 22.84 horses, and the water expended per horse per hour 62.40 lbs. The effective pressure was $\overset{\text{lbs.}}{20} - (\overset{\substack{\text{For friction.}\\ \text{lbs.}}}{2.50} + \overset{\substack{\text{For blast-}\\ \text{pressure.}\\ \text{lbs.}}}{4}) = \overset{\text{lbs.}}{13.50}$; the effective power 15.41 horses; and the consumption of water as steam 92.49 lbs. per horse per hour.

At 30 lbs. pressure per square inch, the absolute power of the engine would be 34.26 horses; the effective pressure $\overset{\text{lbs.}}{30} - (\overset{\substack{\text{For friction.}\\ \text{lbs.}}}{2.50} + \overset{\substack{\text{For blast.}\\ \text{lbs.}}}{6}) = \overset{\text{lbs.}}{21.50} = 24.55$ horses-power. The water expended as steam, at this pressure, would be 1835.95 lbs. per hour, for, if there be one certain property of steam ascertained, it is that its density is proportional to its elasticity; the aqueous constituent of

equal volumes of steam at 30 and 15 lbs. above the atmosphere is, therefore, as 44.71 to 29.71. The absolute horse power would thus be obtained by the hourly expenditure of 53.58 lbs., and the effective power by 74.78 lbs. of water as steam. It will be observed that I have assumed, at this pressure, the same proportional resistance for the blast as was ascertained at 15, and 20 lbs., and, so far, it is a hypothetical case; but, it has been shewn, in the section on the blast, that the principle of that resistance increasing in the same ratio as the absolute pressure on the piston is, within very narrow limits, a necessary condition developed by the phenomena of the blast.

It thus appears that, with increasing absolute pressures, and under increasing loads, an equal effective power is obtained by the locomotive engine with a diminishing expenditure of steam, which proves the truth of Mr. Sylvester's proposition even with the blast; for, at the respective pressures of 8, 15, 20 and 30 lbs., the effective horse-power—in the usual acceptation of the term—is realized with 256.41, 112.54, 92.49, and 74.78 lbs. of water as steam, respectively; at which last pressure it very nearly approaches the economy of the condensing engine.

There is, however, another value necessary for perfect accuracy neglected in the foregoing calculation, viz., the additional friction brought upon the moving parts of the engine by the load, or, more strictly speaking, by the increase of absolute pressure on the piston; for, with the load, the blast-resistance also augments; so that the additional friction is not proportional to the load (as M. de Pambour expresses it), but to the excess of pressure upon the piston over and above that which is necessary to overcome the friction of the engines without load.

Dealing as I am with facts, and ascertained quantities, not with theories, I can assign no value practically found for this additional friction; but, it is nearly certain that it bears a direct ratio to the force which produces friction between those parts of the engine affected by pressure, such as the valves, joints, and slides. For the sake of shewing by how much the neglect of this quantity may influence the results in power, and consumption of water as steam, for a given effect, I will assume $\frac{1}{15}$th—which, from experiments directed to this end, I do not think to be far from the truth—of the absolute pressure, in excess over that which balanced the friction of the engines without load, to be the value of the additional friction, and that it increases in the ratio of this excess. The total friction of 15 lbs. absolute pressure would thus be 3.33 lbs.;

blast resistance, 3 lbs.; effective pressure, 8.67 lbs.; effective power, 9.9 horses; and the water consumed as steam, 123.23 lbs. per horse per hour.

At 20 lbs. absolute pressure, the total friction would be 3.66 lbs.; blast resistance, 4 lbs.; effective pressure, 12.34 lbs.; effective power, 41.08 horses; and the consumption of water per horse per hour, 101.22 lbs.

At 30 lbs. absolute pressure, the total friction would be 4.33 lbs.; blast resistance, 6 lbs.; effective pressure, 18.34 lbs.; the effective power, 20.94 horses; and the expenditure of water as steam per horse per hour, 87.67 lbs.

If this identical engine were put upon a railway, and wheels of 5 ft. diameter applied to its crank shaft, a velocity of 10.71 miles per hour would be attained by 60 revolutions per minute, at which speed the foregoing computations give its power, and effect, for the different pressures of steam; and from the effective—or what would be better termed the disposable—power of the engine, after overcoming all the friction proper to itself, and blast-pressure, the resistance to the progressive motion of a given load could be accurately ascertained, the disposable force being known in horses-power; but, were I to assign 200 tons, or any other load, as the measure of the power of the engine at that velocity with 30 lbs. pressure—for example—a doubt might be raised as to the correct adaptation of such load to the power, and the results might be considered as hypothetical. I prefer, therefore, to apply the same data for the values of friction without load—additional friction—and blast-pressure—to the locomotive engines in Tables VIII., IX., X., in order to exhibit the facility and accuracy with which the sum of resistance to progressive motion can be found when these data are known; and to shew the discordant nature of results afforded by purely theoretical methods of investigation. These results are registered in Table XVI.

The following is an example of the computation; *Hecla*, case 2:—

$\overset{\text{Absolute pressure. lbs.}}{30.95} - (\overset{\text{Friction. lbs.}}{2.50} + \overset{\text{Additional friction. lbs.}}{1.89} + \overset{\text{Blast-pressure. lbs.}}{6.19}) = \overset{\text{Effective pressure. lbs.}}{20.37} = \overset{\text{Effective H. P.}}{88.31}$, or disposable force; $\therefore \overset{\text{H. P.}}{88.31} \times \overset{\text{lbs.}}{33000} = \overset{\text{lbs.}}{2914230}$ raised 1 ft. per min. $\div \overset{\text{Veloc. engine per min. feet.}}{2719.43} = \overset{\text{lbs.}}{1071.63}$ resistance overcome, or tractive effort exerted by the engine; and $\overset{\text{lbs.}}{1071.63} \div \overset{\text{Tons.}}{82}$, gross load, $= \overset{\text{lbs.}}{13.06}$ per ton, at 30.93 miles per hour, instead of 22.40 lbs. per ton, according to Dr. Lardner's data.

Table XVI.

From Table VIII.	Velocity of engine per hour.	† Absolute pressure on the pistons, per sq. in.	Effective pressure on the pistons, per sq. in.	† Absolute power of engine.	Effective power of engine.	Tractive resistance.	Gross load.	Tractive resistance per ton.	Assigned theoretic resistance per ton.	
Experts.	Miles.	lbs.	lbs.	Horses.	Horses.	lbs.	Tons.	lbs.	lbs.	
I.	9.72	49.29	33.82	48.93	33.57	1294.44	206.90	6.25	8.	M. de Pambour.
II.	15.00	29.79	19.52	45.62	29.89	747.25	139.54	5.35	8.	"
III.	18.15	20.00	12.34	37.06	22.86	472.13	52.05	9.07	8.	"
IV.	24.07	13.29	7.41	32.54	18.04	281.05	42.20	6.66	8.	"
V.	31.60	15.30	8.89	42.52	24.70	293.08	41.16	7.12	8.	"
VI.	26.90	20.04	12.37	46.18	28.50	397.55	47.41	8.38	8.	"
VII.	18.63	39.19	26.37	62.67	42.16	848.56	95.41	8.89	8.	"
VIII.	26.70	25.62	16.46	58.72	37.72	529.58	44.08	12.01	8.	"
IX.	21.79	26.62	17.20	49.77	32.15	553.30	57.00	9.70	8.	"
X.	23.00	21.95	13.77	43.33	27.18	443.15	64.36	6.88	8.	"
XIII.	20.34	38.50	25.90	67.24	45.23	833.88	95.41	8.74	‡ 9.60	"
XIV.	29.09	21.54	13.47	53.55	33.48	431.59	45.59	9.46	‡ 10.60	"
XV. 1	26.95	31.04	20.43	104.29	68.64	957.35	82.00	11.67	§ 19.64	Dr. Lardner.
XV. 2	30.93	30.95	20.37	119.52	88.31	1071.63	82.00	13.06	§ 22.40	"
* Soho	30.79	33.30	22.10	107.27	71.19	867.05	46.51	18.64	......	
Ditto.	30.79	51.30	35.29	165.25	113.67	1384.43	46.51	29.98	......	
1	2	3	4	5	6	7	8	9	10	11

Columns 9 and 10 of this Table bring the resistances opposed to progressive motion, calculated from data of a practical nature, into comparison with those deduced from experiments of a purely theoretic character; but, there are good grounds to believe that the effective, or disposable power of the engines in the Table is yet too highly rated; for, the friction of these engines, without load, would be greater than that assigned, as their pistons are of larger diameter than those upon which the value of friction was ascertained to amount to $2\frac{1}{2}$ lbs. per square inch. It is probable, too, that the additional friction would be

* From Table XIII. † Vide columns 29 and 33, Tables IX. and X. ‡ Vide page 97.
§ Vide pages 112, 113.

greater than $\frac{1}{15}$th in the railway locomotive, from the shocks and strains to which it is exposed. It must, also, carefully be borne in mind that the only datum whence the power can be derived in these experiments, arises from the calculated ratio of the volumes of steam and water consumed; and, that though the former may have been ascertained with near approach to certainty, the second important quantity has been shewn, in many of the reported cases, to exceed possibility; thus, it is probable that in no one of these experiments has sufficient allowance been made for waste, priming, and delays: the absolute pressures so found must, therefore, be regarded as too high, rather than too low, and, consequently, the resistances to motion exaggerated.

Extraordinary discrepances appear by mutually comparing the velocities, and resulting resistances; they forcibly shew the importance of an exact appreciation of the volume of water consumed, when no other test than its ratio to the volume of steam is used to denote the actual pressure in the cylinders. Great as is the difference between the results with the *Hecla*, and Dr. Lardner's estimate of resistance for the velocities, the contrast between Experiment V. (at a still higher speed,) and that engine, exhibits the tractive effort required, as less by nearly 6 lbs. per ton at the higher, than the lower velocity. Reasons have already been given in the analysis of the experiment with the *Hecla*, for considering the effective power of the engine as too highly rated, after the deduction of water for delays, &c., as no estimate could be formed of the waste through the safety valves. The resistances in columns 7 and 9 of this Table are, therefore, still exaggerated.

The power of the *Soho* has been computed at two pressures; 1st, on the presumption that all the water assigned as evaporated, was expended as steam; 2d, on the presumption that $\frac{1}{4}$th was wasted, as by M. de Pambour's approximation; and it will be seen that the deduction of $\frac{1}{4}$th water has the effect of reducing the absolute pressures, and disposable force of an engine in a much greater ratio; an instance adduced to impress upon observers, and experimenters, the all important necessity of an accurate determination of the expenditure of water as steam. With the deduction of $\frac{1}{4}$th, the force of the wind against this train raised the resistance per ton to 18.64 lbs., or $5\frac{1}{2}$ lbs. higher than in the experiment with the *Hecla* at equal velocities. On the presumption that all the water passed as pure steam through the *Soho's* cylinders, the pressure on the pistons must have been 51.30 lbs. per square inch, which I look upon as a very improbable circumstance, and the resistance to progressive mo-

tion 29.98 lbs. per ton—a still more improbable circumstance. The pressure—with $\frac{1}{4}$th water deducted—would be 33.30 lbs. per square inch.

I have reduced several of the experiments on the engines of the Great Western Railway to the terms of this last Table, but the computations are too laborious to tempt a reduction of the series, unless the waste of steam, and pressure in the boilers were denoted; for, as in Experiment XI., Table VIII., the pressures resulting from the ratio of the volumes of water and steam consumed come out, in many cases, much higher than the boiler itself could have sustained, so that, without correction for waste, computations from such data would give no credible result as to power, or resistance.

An accurate determination of resistance to progressive motion can only be made from correct facts establishing, beyond dispute, the disposable force of the particular engine examined; no such facts are yet on record, and until they be ascertained with the same care, and precision, as upon other classes of engine, doubt and confusion must rest upon investigations which, otherwise, could not fail to indicate and illuminate a path to improvement in locomotive science; and it must be almost unnecessary to say that upon the constructive perfection, and economy of the locomotive engine depends the prosperity of railways; for, the locomotive is the continual devourer of costly power, and the continual object of costly repair.

JOSIAH PARKES.

London, 1839.

This paper was partially read on the last evening of the session. It contained the greater portion of the facts and Tables, and all the principles now developed; but as M. de Pambour's last work, the "*New Theory of the Steam Engine*" was not then published, and as the meeting of the British Association for the Advancement of Science might be expected to produce information on the subject, I obtained permission from the Council of the Institution to incorporate in my paper such matter as might seem expedient; for which indulgence I have to express my sincere thanks.—J. P.

III.—*On the Preparation, Properties, and Uses of Turf and Turf Coke.*

By CHARLES WYE WILLIAMS, Assoc.Inst.C.E.

Having laid before the Institution specimens of turf in its natural state, as taken from the bog; also specimens exhibiting certain stages of its conversion into a dense coke, or into the form of a solid artificial coal; I now propose to submit some observations on the composition, properties, and respective uses of these different substances.

My connection with the City of Dublin Company rendered me extensively acquainted with steam navigation, and instrumental in introducing it into Ireland, in aid of inland intercourse on the river Shannon; my attention was consequently drawn, several years since, to the substituting turf for coal, as a fuel for steam vessels—principally on the score of economy and convenience—coal being obtained with difficulty, and at a great expense; while turf abounded in numerous districts along the hundred miles of that river over which the steam vessels of the company daily passed. A further inducement was, that its adoption, as a fuel for steamers, would form a valuable and profitable source of employment for the population. The result of the trial has been satisfactory in every point of view.

In the adoption of a turf fuel, no small inconvenience was experienced from its great bulk; and, in wet seasons, from its retaining so much moisture as seriously to detract from its heating powers. My attention was, therefore, first directed to the remedying these two evils, by obtaining a more condensed and a drier fuel.

In pursuing this object I was struck with the meagre accounts which books afforded of this valuable natural product, and the little attention which had been given to it by scientific and practical men in this country. My humble attempt is to remedy this defect.

As to the means of increasing its density, and thus remedying the evil of its excessive bulk, nothing had been effected; neither had any successful effort

been made towards improving the mode of preparing it for the purposes of fuel, notwithstanding the great importance of these objects.

My attention was further drawn to the value of turf (or peat fuel, as it is called in England and Scotland) by the received opinion, that it had not only the power of giving out an intense heat, and with great rapidity; but that it possessed properties which gave it great value when applied to the various processes of metallurgy; and particularly in the working of iron, where the fuel comes in contact with the metal. This led me to pursue the enquiry on another ground; namely, as being likely to supply an improved fuel for the uses of the furnace and the forge.

The well known superiority and high money value of "*Charcoal iron*," (iron manufactured by means of the heat from charcoal, and which is the leading peculiarity in the manufacture of Swedish iron,) gave a further stimulus to the enquiry. Coke prepared from turf, (a pure vegetable charcoal,) ought, it appeared to me, not only to possess heating properties analogous to those from wood charcoal, but to be equally free from those deleterious ingredients which abound in mineral coal.

Such, indeed, is the value and purity of the iron manufactured by the aid of wood charcoal, as compared with that manufactured by coal coke, the process usually adopted in Great Britain, that it may be hoped that by the aid of turf coke the importation of Swedish iron may ere long be rendered unnecessary.

Of the use of turf coke in working iron, many strong testimonials from practical men were given in a tract presented to me by Lord Downshire, whose attention had long been directed to the means of rendering the Irish bogs more available. Its importance for the uses of the forge cannot be overlooked. Much injury is sustained, not more from the use of inferior iron, than from the impurities of the coal and coke with which, in many parts of the country, we are compelled to work it. The extent of this evil and its consequences, cannot, it is true, be stated in figures, but it is not less appreciable on that account. This is well known to all workers in iron and steel; and, when we find an important part of a machine broken, and probably great mischief done, we are apt to censure the workman, when we should rather lay the fault on the iron, or the impure fuel with which the work has been done.

In pursuing the enquiry as to the use of turf and the manufacture of turf coke, I fell naturally into the common error of taking the lower portions of the

bog, in preference to those nearer the surface; and from this circumstance, that the latter, on account of their lightness, appeared unsuited to the purpose; while the former, from their great comparative density, seemed well adapted for producing a coke which could stand the blast. From the lower strata, a coke sufficiently dense could certainly be formed by the aid of suitable coking stoves; but they were found to be too impure, and impregnated with too large a proportion of incombustible and deleterious matter. From the upper strata, on the contrary, and particularly where they were composed of pure bog moss, which had made but little progress towards decomposition and solidification, I obtained an exceedingly pure carbon; giving a very small per centage of useless, and no injurious matter.

This upper portion of the bog, however, was of so light and porous a texture, and so apt to reabsorb moisture, by which its heating properties were much reduced, that it threatened to defeat all my efforts, as it would scarcely repay the labour of cutting and saving, even for domestic fuel; while the lower strata often approached the solidity of coal. The superior density of the latter, it will be observed, had been acquired by the decomposition, and consequent solidification of its vegetable fibre, and still more by the consolidation, through ages, from the mere pressure of the superincumbent mass; often of the depth of twenty or thirty feet. But this great density is obtained at the expense of purity and heating properties, through the addition of many heterogeneous and incombustible substances; which, *pro tanto*, and without reference to their chemical effects, deteriorate the calorific power and usefulness of turf as fuel.

This has been well illustrated by Mr. Griffith, in his analysis of a part of the Bog of Allen, of the depth of thirty-eight feet, as given in the first volume of the "Parliamentary Reports on the Bogs of Ireland." That report states that the upper portions, even to the depth of eight or ten feet, exhibited so "open-grained and fibrous a texture," that the different species of the mosses of which it had been composed were easily discernible: the *Sphagnum palustre* (the lightest of the bog mosses) predominating. This portion of the bog had as low a specific gravity as 356 (water being taken at 1000); and, what is here important, yielding not more than one per cent. of incombustible ash.

As he descended to the lower portions, he found the mass progressively increasing in density, until it shewed a "fracture, conchoidal; lustre, shining;

with a strong resemblance to coal, and susceptible of a high polish," and further, that it was capable of yielding a "very compact charcoal with internal lustre shining." He found its specific gravity increased from 356 to 1236, but accompanied with the drawback, that its incombustible ash had also increased from one to twenty per cent., independently of the injurious tendency of the substances with which it had combined; thus proving that as the bog had gained in density, it had lost in combustible value, weight for weight; and that even as a domestic fuel it was seldom used, "owing to the unpleasant odour it gave when ignited."

Having ascertained satisfactorily, by experiments in the forging of iron, and by analysis, that the upper and lighter portions of the bog possessed the greatest purity and heating power, weight for weight; the difficulty presented itself of combining density with purity, which qualities, in the natural state, are not co-existent. The process I have adopted has enabled me to obtain a coke from the lighter portions of the bog, not only of double the density of wood charcoal, and equal to that of coal coke, but possessing that purity which is so essential in the working of iron.

Analysis of peat and peat coke.

To ascertain more in detail the relative values of the compressed peat, and peat coke, as compared with coal, coal coke, and charcoal, I had a very accurate analysis made by that able experimentalist, Professor Everitt, whose report I here subjoin, as it contains much interesting matter, and many new facts.

"*Report of Experiments on pressed Peat, and on Coke made therefrom, for Charles Wye Williams, Esq.*

Density.—The density or specific gravity of water			1000
Compressed peat, the thinnest and hardest pressed			1160
Ditto, the thicker or less pressed			910
Peat coke, the thinner or hard pressed			1040
Ditto, the thicker or less pressed			913
The resin fuel, (peat, coke, and resin)			1140
Resin alone			1110
The hardest and dry woods, such as oak, ash, or elm, vary from	800	to	585
The lighter woods, as poplar, pine, &c., vary from	383	to	530
Charcoal from hard woods, varies from	400	to	625
Coals vary from	1160	to	1600

"Hence we see, the hardest compressed peat is denser than the hardest woods, in the relation of 1160 to 885; and compared with some of the lighter woods, nearly double. Further, that the coke prepared from the hardest compressed peat is nearly double the density of ordinary charcoal. In common practice, it is reckoned that 100 lbs. of charcoal occupy the same space in measure as 200 lbs. of coke. The peat coke would, therefore, weight for weight, occupy the same space, very nearly, as common coke.

Calorific power.—"The next point of investigation was the calorific power as compared to coal, common coke, and charcoal. The usual method of making assays of this kind, is to burn equal quantities of the respective fuels, and endeavour to ascertain how much water each respectively will raise a given number of degrees, or convert into vapour. But experiments of this sort, unless made on a very large scale, cannot lead to any comparable results.

"It is given in Berthier (*Essais par la Voie Sèche*, vol. i. p. 289) as being the result of accurate experiments, that a given weight of charcoal will raise 78 times its own weight of water from 32° to 212°, or boil off in vapour $11\frac{8}{10}$ths its weight; which data do not differ materially from the results obtained on a large scale by Mr. Parkes. (See his paper in the "Transactions of Civil Engineers," vol. ii. p. 161.) Now we know, from actual trial, that weighed portions of coke, charcoal, &c., used under stills and boilers holding from five to ten gallons of water, will not produce one tenth of this effect. I am convinced of the utter futility of trusting to any such experiments on a small scale, with the view of having anything like an approximation to the true relative values of fuel. I was here induced to adopt the method recommended by Berthier in his work, vol. i. p. 228, in order to obtain the relative values of these fuels.

"It is assumed that the absolute quantity of heat generated during the combustion of any fuel, is in exact relation to the quantity of oxygen consumed, or entering into combination. Hence, in order to ascertain the relative calorific value of fuels, it is only necessary to ascertain the quantity of oxygen each consumes in burning.

"The best mode of doing this, is to mix a weighed quantity of the fuel with a slight excess of litharge, (oxide of lead,) and find what quantity of *metallic* lead is reduced. According to Berthier, (and which also agreed with some trials made by me on the same substances,)

10 parts of pure carbon will give, of lead		. . .	340 grs.
10 parts of good wood charcoal	from	. .	300 to 323
10 parts of dry wood . .	from	. .	120 to 140
10 parts of good coal coke .	from	. .	260 to 285

"It may here be remarked that, assuming the principle to be correct in practice, it is susceptible of great accuracy; for, as every *single grain* of carbon produces thirty-four grains of lead, any error in estimating the lead is reduced to $\frac{1}{34}$th of a grain of pure carbon.

"In subjecting the peat and peat coke to the same process, I found as follows:

10 parts of the peat coke (picked surface peat) gave .	277 grs.
10 parts of peat coke (lower strata)	250
10 parts of pressed peat (uncoked)	137

"The above numbers represent the relative quantities of heat which can be produced by the same quantities of each of the fuels; and in cases where quantity of heat alone is the consideration, these numbers will also represent their relative values.

"But *intensity* of heat is often of more consequence than *quantity;* and intensity depends very much on the *density* of the fuel. Thus, charcoal can never produce so high a heat as coke; and, in this respect, the denser peat coke and common coal coke are about equal. These comparisons are quite irrespective of any foreign matter being present which may be injurious to the quality of iron, when the fuel is used for reducing the metal from its ore, or for working iron by fire, or when it is used under iron boilers for generating steam.

"To see how far it was probable or not that the peat coke contained matter likely to act injuriously in this respect, like some coke, I burned portions of it in a variety of ways, but no sulphurous acid smell could, in any case, be perceived: now, sulphur, or the metallic sulphurets, are the usual ingredients in common coke, to which their corrosive effects on iron boilers is to be attributed; and such coke, during burning, always gives perceptible quantities of sulphurous acid gas.

"As the nature and quantity of ash is sometimes of importance, I have also investigated these points with great care. An average of two experiments, where 1000 grains of peat coke were burnt till all carbonaceous matter was consumed, gave $\frac{5}{100}$ for the quantity of ash of a light buff colour.

100 grains of such ash contain—

Common salt	3·5
Silica and sand combined	15·0
Sulphate of lime	22·5
Carbonate of lime	43·25
Magnesia and carbonate of magnesia	15·00
Alumina	0·75
	100·00

"The ash contained no carbonate of potassa, and is remarkable for the large quantity of magnesia present.

"From my trials I am of opinion, 1st. That the peat coke examined by me contains nothing which would, during the burning, be more injurious to iron than wood charcoal, or the best coke—whether it be used to work iron, or under boilers for the generation of steam.

"2d. That it is equal to the best coke, weight for weight. In heating power a little inferior, weight for weight, to wood charcoal where quantity is the only consideration; but where bulk of stowage and high intensity of heat are important considerations, it is superior to wood charcoal.

"THOMAS EVERITT,
"Laboratory, Medical School,
"Middlesex Hospital."

"LONDON, JAN. 18th, 1839."

Observations on report.

The turf which I submitted for the above experiments was from a bog in Lancashire. But, from other experiments, I find the turf from many of the bogs of Ireland exceed it in purity, and contain a much less proportion of incombustible ash.

In considering the foregoing report and analysis, the great density of both the peat and peat coke, though produced from the lighter portion of the turf, from the surface of the bog, is remarkable; the compressed peat being thirty per cent. denser than oak wood, and double that of lighter woods; while the coke is double the density of charcoal, and on a par with coal coke. I may here add, that this density, which is so valuable where intensity of heat is an object, may be still further increased, and with little additional expense, by continuing the operation of pressing.

This being the first time that the results of the *litharge test*, as applied to turf coke, have been communicated in this country, (the value of which, Berthier,

in his elaborate and admirable essay on combustible bodies, has fully established,) I may be permitted to say that its accuracy, and the small amount of practical error to which the process is liable, as shewn by Mr. Everitt, give it a high claim to our attention; although to persons not familiar with the nature of chemical tests, it may not be so self evident. We here see that the extraordinary attraction which carbon has for oxygen, and the power which it thereby exercises of deoxidizing metallic oxides, renders the litharge test the most suitable for determining the absolute purity and calorific powers of the various cokes. The carbon, under a high temperature, uniting with the oxygen in proportion to its calorific powers; while the lead, being thus deprived of that which is essential to its state of oxide, is precipitated in its pure metallic form:—the relative weights, so thrown down, representing the true combustible values of the several cokes.

It will be particularly observed that Mr. Everitt, in stating the quantity and intensity of the heat given out by peat coke, adds, that these are "irrespective of the presence of any foreign matter which may be injurious to the iron." Now we know that many foreign substances do enter into the composition of coal and coke, and do exercise a very injurious influence over iron and steel, in the furnace and forge. In this respect the importance of the peat coke becomes apparent. Iron, when worked by means of this coke, is not only sooner brought to a welding state, but it is found to work softer and with less of that scaling which is so injurious, particularly in the operation of welding. These facts I have proved both in the furnace where large boiler plates are heated, and in the operations of the forge, when even the worst iron was improved in quality.

It is not an unimportant consideration that peat coke may thus be produced from that portion of the bog which has ever been rejected even as a domestic fuel. Again, that it is precisely that description of turf which most abounds in Ireland, and which, in most of the large bog districts, has hitherto been regarded as an absolute incumbrance; alike unfit for fuel, and for conversion to agricultural purposes. This latter arises from its extreme porousness and levity:—its being so far removed from that decomposition which is essential to the vegetative functions of all soils:—and also its susceptibility of the extremes of moisture and drought,—overcharged in wet seasons, and amounting to a mere *caput mortuum* in dry ones.

Tredgold, speaking of turf, says, "its specific gravity varies considerably, and when free from gravel, its quality as a fuel depends much on its *density*.

As a fuel, it may be divided into two kinds; the first is compact and heavy; the second light and spongy"; and of the former, he adds, "this is the best kind." Again, he says, "Peat affords a mild and gentle heat, but it is not a good heat for supplying furnaces for boilers." Now from these observations it is manifest Tredgold experimented on some of the lower strata, which give out an unpleasant odour in proportion to their earthy impurities. It is also evident he had not analysed it with the view of determining the relative purities of the light and heavier kinds, and of its peculiarities in the working of metals, or he would have arrived at directly opposite conclusions.

Tredgold adds, "according to Clement and Desormes, it affords one-fifth the heat that would be given out by an equal weight of charcoal, which nearly coincides with the ratio of Blavier and Miché." This comparison is, however, an incorrect one, as raw turf is here compared with charcoal. I do not, however, hesitate to enter the lists with these celebrated men, and assert that by proper management, turf, even of the lightest description, will be found to give out more effective heat than wood, and turf coke than charcoal. Where, then, the required density can be obtained artificially, we are in a position to avail ourselves of this substance, which has hitherto been so rejected, (merely from its low specific gravity,) and render it available, as a fuel, where high temperatures are required, and particularly in all the operations of metallurgy, where purity of heat is so essential.

Process of manufacture.

The difficulty of the process of converting turf into coke, has hitherto been an obstacle to its application to the uses of the forge. So great is the attraction it has for oxygen that much care is requisite in managing the process: on the one hand, to deprive it of all volatile substances, and convert it into a pure carbon: and on the other, to avoid waste from partial combustion.

The mode by which I have effected these two objects is by the use of large vertical stoves, combining in their action the distillatory with the stifling process. During the expulsion of the volatile substances, a species of distillation is carried on in the ovens; and when charred to the point when combustion would begin, the stifling process is adopted, by which all waste is avoided. The peculiarity of the process effected by these stoves, consists in watching the progress of the coking, which begins at the lower part of the stove, and so regulating the admission of the due quantity of air, as the operation ascends, until the whole is so advanced and the gases so expelled, as to require the

complete exclusion of atmospheric air; the whole is then suffered to cool by mere radiation from the external surface of the stoves.

Each of these stoves is capable of coking a ton weight of turf at a charge, and twenty-four hours are requisite for the operation. Their number may be increased according to the quantity required. In that case, they may be advantageously built in a range, or combined in a hexagon shape, by which economy of space and materials in their construction is obtained. When so placed, the coking and roasting stoves (of which latter I shall speak presently) may be alternated with great advantage in the economy of heat.

In the preparation of turf coke for the purposes of the forge or melting furnace, a further process is necessary to give that density which is so essential. This density is obtained by pulverizing, or bruising, the raw material in a wet state as taken from the bog, and previously to compression. This operation of bruising, may be effected with very little labour, by proper machinery. The effect of this bruising process, is, by the destruction of the fibrous character, to bring the component parts into closer and more intimate contact; the result of which is observable in the high specific gravity thus obtained.

By the union of these two processes, the pulverizing and pressing, almost any desirable density may be obtained; and we have within our reach the power of imparting to this valuable substance, the most essential properties which fuel can possess; namely, the *purity* of vegetable charcoal, with the *density* of mineral coke.

Berthier, in his essay on combustible bodies, and in which he has given elaborate analyses of various kinds of charcoal, coal, and turf, with their respective cokes, observes, Vol. I. p. 347, "With respect to intensity of heat, there is no combustible equal to coke. It produces results in large furnaces which could not be obtained from wood charcoal. In the furnaces for assaying of metals difficult to be melted, it raises the temperature ten pyrometric degrees higher than the latter. This property is owing to its great density."

But Berthier does not appear to have been aware of the possibility of giving to turf coke, a density equal to that of coal coke, which he thus eulogises. I find in the trials made by him on numerous kinds of turf, there were none in any degree approaching to the density of the fuel I have produced.

Believing, then, that the quality of the iron ore, is not a matter of greater importance than the quality of the fuel by which it is reduced to a metallic

state, it becomes a subject of great interest, in many of the arts and manufactures of this country, to enquire into the nature and properties of turf coke.

Nearly thirty years ago, Mr. Griffith, in the Parliamentary Reports on the Bogs of Ireland, characterised those bogs, as "mines above ground." In one of these reports, under date June, 1810, he makes use of the following, I trust prophetic, observation: "It is not at all improbable that the iron founders and other manufacturers in Dublin, who have occasion for a great heat, quickly raised, may, at no distant period, be supplied with turf charcoal (which is superior to any other) for that purpose, from the Bog of Allen. Many interesting experiments have been made in France on this substance, in most of which the result has answered beyond expectation." From that period, however, to the present, nothing has been done towards availing ourselves of those "mines above ground," or turning this valuable substance to any useful purpose beyond that of a domestic fuel.

As the several uses to which peat and its compounds may be used are more numerous than is generally imagined, I will here detail a few of the leading ones, with some observations on their use and application:—

1st. Of turf, as now prepared and sold in Ireland.
2d. Of this description of turf, after dry pressure.
3d. Of the same, after the operation of roasting.
4th. Of the same, after having been converted into coke.
5th. Of the preparation of turf for the manufacture of gunpowder.
6th. Of a denser kind of turf coke, prepared by grinding.
7th. Of turf coke, applied to the purification of coal gas and coal coke.
8th. Of turf coke for locomotive engines.
9th. Of the same, applied to the manufacture of artificial coal, with bitumen, for steam navigation.

1st. Of turf, as cut and sold by the peasantry in brick-shaped pieces.—The first practical impediment to the use of turf in this state, for the generation of steam, is the great quantity of moisture it contains. The second inconvenience is its bulk. As regards the moisture held mechanically in suspension, this is an evil of much greater extent than is generally supposed; and as the loss of effective heating power must be in the proportion of internal moisture which it has to evaporate, the heating properties of turf saved in the ordinary way is not unfrequently deteriorated to the extent of fifty per cent. Indeed, in wet

seasons, particularly when we have to deal with the light spongy descriptions, from their power of reabsorbing moisture, it is often impossible to maintain the due pressure of steam for the proper working of the engines, even by the sacrifice of any quantity of turf.

For this evil the great remedy is, attention during the process of drying and saving; and care in its subsequent preservation for the uses of the ensuing season. Turf should be stacked under cover, a circumstance hitherto wholly neglected by those who lay up annual stocks for the winter months, as in the case of the distillers in Ireland. Any labour or expense, therefore, devoted to this object, will be amply repaid by the diminished quantity consumed, and the increased efficacy of its evaporative powers *.

2d. Of turf, as now prepared in Ireland, after pressure.—With respect to its great bulk and low specific gravity, this may to a certain extent be remedied, and at an expense which will repay the process. When thoroughly dry, turf may be compressed, in large quantities, at a moderate expense, into one-half, or even one-third of its previous bulk: and which compressed state it will retain, provided it be preserved under cover. This was not hitherto supposed to be the case; pressure has not been applied to turf in this previously dry state, as it was supposed that, like a sponge, it would again swell and resume its former bulk. My experiments on this preparation much surprised me, and led to the manufacture of a valuable description of fuel condensed at a cheap rate, and with the aid of dry pressure alone. Where turf is of a light porous character, and where its bulk is necessarily more objectionable, this dry com-

* The following extracts from Berthier's "Traité des essais par la Voie Sèche" will be found much to the point on the subject:—

"Hygrométricité.—Le bois est un corps extrêmement hygrométrique: il absorbe une très grande proportion d'eau dans l'air humide, et lorsque ensuite on le soumet à une chaleur graduée, il abandonne cette eau par portions successives qui dépendent de la température. Selon M. Karsten, des copeaux de chêne parfaitement desséchés à l'air, perdent encore 0,103 de leur poids, à la température de l' ébullition; mais à cette température le bois retient encore une quantité très notable d'eau." Vol. I. p. 234.

"Dessication.—Si la dessication n'a lieu qu'à la chaleur de 100°, la matière retient encore 0,145 d'eau non combinée; mais en portant la chaleur jusqu' à 150 ou 160°, et en la maintenant pendant quelques heures, jusqu' à ce que la poudre commence à brunir, on a le ligneux parfaitement sec; dans cet état il est composé de parties égales de carbone et d'eau, ou plutôt, d' hydrogène et d' oxigène dans le rapport rigoureusement propre à constituer l'eau. Ce résultat est tout-à-fait conforme à celui que MM. Gay-Lussac et Thénard avaient obtenu il y a déja long temps." Vol. I. p. 241.

pression is of great value; as it will be found to have greater durability in the furnace, than would appear due merely to its diminished bulk.

The lower heavy strata, however, cannot advantageously be compressed, as they have a tendency to crumble and lose their consistency; being in fact to a considerable extent already carbonized. The upper and middle strata may, when thoroughly dry, be compressed under an hydraulic press, in which state they are applicable to many purposes. We know that mere density, in wood, has no relation to the quantity of heating power which it is capable of giving out weight for weight; and when equally dry, the lighter kinds of woods give the same heat as the heavier kinds. The same law applies to turf, keeping in view, equal purities,—equal weights,—and equal degrees of dryness. These are important facts when we have to deal with a light, though pure material, such as the upper portions of turf bogs.

3d. Of turf after the operation of roasting.—When it is an essential object to have highly dried turf, this may be obtained by the roasting process, in the vertical stoves, so arranged that a strong current of air is made to pass through the turf, entering at the bottom and passing upwards, escaping at the top and carrying away the moisture. As a high temperature and an adequate current of air are essential to the evaporation of moisture, these drying or roasting stoves may be placed alternately, as already mentioned, with the vertical coking stoves. The heat thus supplied from the latter will be found sufficient to effect the necessary rarefaction of the air, in the roasting stove; whereby a rapidly desiccating heat and current are obtained, without any cost. I have also found this compressed roasted turf to be well adapted for many kinds of furnaces.

The object of this roasting, is not only to expel the moisture which cannot be dislodged by mere atmospheric exposure, however dry the air may be, but also to vaporize and expel the ligneous acid which abounds in the lighter description of turf. This acid being a great enemy to heat, must be thoroughly dissipated, if we would obtain the greatest calorific effect from the turf. This very circumstance having been hitherto disregarded, has in many instances been the direct cause of its heating properties being underrated. Berthier, who has treated of the relation between desiccation and calorific power with great exactness, states, that having dried wood in a stove during many days at a high temperature, and, further, until it had the appearance of roasted coffee, found

that the calorific power increased progressively as this species of distillation proceeded, up to the degree of heat capable of decomposing it.

Rumford, with the view of thoroughly drying it, cut the wood into thin pieces, and exposed it in a stove for twenty-four hours, and until it began to turn brown, at a temperature of 128°. In all these particulars I have ascertained that turf and wood are perfectly analogous.

Dr. Ure, in speaking of the mischievous effect of moisture in wood, explains it in his usual clear, comprehensive, and satisfactory manner: and how forcibly does all this apply to the moist state in which turf is now usually applied as fuel; yet how indifferent are the consumers to this self-evident deterioration of the heating properties of their turf *.

4th. Of turf coke.—This dry-pressed turf, when coked, forms a valuable fuel, particularly for the use of air-furnaces, in which iron plates for boiler-makers are heated. In the manufacture of marine boilers, and particularly for boilers made on the Cornish plan, many of the plates of iron forming the windings of the internal flues, have to be brought to a red heat above 50, or even a 100 times, before they are fashioned and fitted to their proper places. It is needless to mention the injurious effects of bad sulphurous coal or coke during such repeated heatings, or the advantage of a pure carbonaceous coke, like peat coke, by which, from its freedom from impurities, the quality of the iron is positively improved. I can speak from experience on this head, having had abundant proofs of its value, and having found that the iron heated in a furnace by this description of turf coke, has become softer, more pliant, and malleable.

5th. Of turf, for the manufacture of gunpowder.—The lighter portions of

* Dr. Ure, in speaking of moisture in wood, observes, "Moisture diminishes the heating power in three ways :—by diminishing the relative weight of the ligneous matter :—by wasting heat in its evaporation :—and by causing slow and imperfect combustion. If a piece of wood contain, for example, 25 per cent. of water, then it contains only 75 per cent. of fuel; and the evaporation of that water will require $\frac{1}{28}$th part of the weight of that wood. Hence the damp wood is of less value in combustion by $\frac{8}{28}$ths or $\frac{2}{7}$ths, than the dry. Woods which have been felled and cleft for twelve months still contain from 20 to 25 per cent. of water. It may be assumed, as a mean of several experimental results, that one pound of *artificially* dried wood will heat 35 pounds of water, from the freezing to the boiling point: and that a pound of such wood as contains from 20 to 25 per cent. of water will only heat 26 pounds of such water to the same degree. The value of different woods for fuel, is inversely as their moisture; and this may easily be ascertained by taking their shavings, drying them in a heat of 140° Fahrenheit, and seeing how much weight they lose."

the turf, when coked, and ground very fine, and which is effected with great facility, appear peculiarly applicable in the manufacture of gunpowder. This is owing to its purity and freedom from earthy matter, and also from the extreme pulverulency of which it is susceptible.

Having submitted this coke to the examination of M. D'Ernst, the artificer in fireworks for Vauxhall, his report is very satisfactory. He states that it is twenty per cent. more combustible than oak charcoal, and recommends its adoption in the manufacture of gunpowder. The turf coke was put to the severest test, namely, in the production of coloured lights.

6th. Of the dense, wet-pressed turf coke.—Where a very dense coke is required, an additional process becomes necessary in the preparation of the turf. As already noticed, turf for this description of fuel is taken wet, as cut from the bog, and in that state, ground or pulverized, so as completely to destroy its vegetable fibrous texture, and break up the numerous cells or vesicles in which the moisture is contained. This bruising process may be effected in the common horse mortar-mill. When a large quantity is required, the number of bruising rollers may be increased on the horizontal arms. A mill of twelve feet diameter, mounted with four metal or stone rollers, working vertically on their edges, is capable of preparing ten tons daily. The only attention required is that of one man to supply the floor of the mill with the wet turf, from which, after having made a few circuits of the mill, it passes out sufficiently pulverized for the operation of pressing.

On being so bruised, and while in its wet pulpy state, it is subjected to a powerful pressure, in layers of three or four inches thick, with the proper alternate absorbents or recipients; and on the water being pressed out, it is reduced to the state of thin cakes according to the degree of density required. These cakes are then prepared for the coking stoves by a short exposure to the air, under cover, or by means of the roasting stoves already mentioned.

An hydraulic press of 400 tons pressure can effect the proper compression of one ton of peat per hour. The charges of turf are previously prepared apart, on suitable trucks, and rolled under the press successively as the pressure is completed. The object of this arrangement is to save the time of the press, which would otherwise be wasted were it to stand idle while the turf was being placed in its proper layers.

7th. Of purifying pit coal by means of turf.—Another advantage, and one likely to be of great importance in the arts, arises from the purifying effect of

turf on pit coal, in the process of coking the latter, and in the manufacture of gas for illuminating purposes.

By mixing turf with pit-coal in the coking oven, a remarkable effect is produced, and a very superior coke is the result. The sulphur, which abounds in coal, and which vaporizes and passes away at a temperature even below the boiling point of water, is first expelled by the direct radiation from the turf, which begins to carbonize and give out its heat long before the coal.

Another valuable effect is produced by the union of the ligneous acid of the turf with the volatile alkali, (ammonia,) which is generated in the process of coking coal, and which is always found where nitrogen (its main constituent) is disengaged in connexion with a combustible containing hydrogen. In the process of coking coal, ammoniacal gas is evolved in large quantities. When carried on in close retorts, this ammoniacal gas passes away in a liquid state, being combined with aqueous matter, and forming the ammoniacal liquor of the gas works.

Now ammonia is not only a great enemy to flame and heat, but causes a serious injury to iron. The ligneous acid of the turf unites with and neutralizes the ammonia; greater purity, and consequently increased calorific effect, is thus given both to the coke and to the gas.

Dr. Ure states that, "the gas from coal, as it issues from the retort, is not fit for the purposes of illumination, as it contains, with other matter, steam impregnated with the carbonate, sulphate, and hydro-carburet of ammonia." If not suited for the purpose of illumination, neither is it suited to the purposes of heating and evaporation. The ammonia being dissipated in the early process of coking, is an important circumstance, from its being thus prevented entering into those numerous and injurious compounds of which it is susceptible; or of carrying off any sensible portion of carbon or hydrogen, with which it would readily unite.

As ammonia in combining with acids generates cold—frequently intense cold, its abstraction is on this account desirable. Professor Brande informs us, that "ammonia mixes with the gaseous acids in several proportions; combining with half its volume,—or with an equal volume,—or with two volumes:" an ample range is thus obtained for effecting this union of the ligneous acid with the ammonia through all the stages of their generation in the ovens. That this is the fact, is perceptible in the dense white cloud, (the form which it assumes,) and which passes off copiously during the whole of the operation.

Had it been otherwise, it would have remained until a more advanced stage of the process, when both the turf and coal would have been carbonized, and in which case the superior affinity which carbon has for ammonia, in preference to all the other gases, would have resulted in a serious loss—the carbon. Now carbon is the most important element in all fuels, being, as Dr. Ure appropriately terms it, "the main heat-giving constituent." Its absorption and loss are, therefore, above all things to be guarded against. The investigation of these interesting details remains yet to be pursued by chemists.

8th. Of turf as a fuel for locomotive engines.—The turf coke, in both states of compression, is expected to be applicable to the use of railroad engines, particularly where coal coke is either expensive or inferior in quality. Much must, however, depend on the money value at which it can be produced; and this, also, must have reference to the locality where it may be manufactured. The application of turf coke to locomotive engines has not hitherto been practically demonstrated, but doubtless will in the course of the present year. Ordinary coal coke exercises a most injurious influence on the tubes and fire-boxes of locomotive engines.

9th. Of turf coke in the manufacture of artificial coal.—The last application or modification of this material to which I shall allude, is in the formation of an artificial coal by its union with bituminous matter, as a fuel for steam navigation. This fuel is not a compressed turf, as has been erroneously stated. It is, in fact, an artificial coal, composed of the best ingredients of which pit coal is formed by nature, yet free from any admixture with those substances which deteriorate the heating properties of the latter. This artificial coal is composed of turf coke combined with resin or pitch, according to the desired quality; the former, of course, making the strongest and best fuel.

Natural mineral pit coal we find composed of, 1st, bituminous matter;—2dly, carbonaceous matter;—and, 3dly, of foreign matter of various kinds, and in various proportions, among which are sulphur and the metallic sulphurets, and incombustible earthy substances. In the composition of artificial coal, I have taken resin, as the purest available bitumen, and turf coke, as the purest vegetable carbon. The result of their chemical, or, perhaps, only mechanical union, is a coal, possessing the greatest heating powers in the smallest bulk—avoiding, on the one hand, an excess of bitumen and a deficiency of carbon, as is found existing in cannel coal,—or, on the other hand, an excess of carbon and a deficiency of bitumen, as is found to be the case with anthracite coal;

neither of which, for these reasons, is adapted for the furnaces of marine boilers.

Resin, notwithstanding its high price, is now much used in steam navigation, from its containing so much heating power, and giving out so much flame; it is only used, however, by mixing it with fine coal or cinders, as, alone, it is not an available fuel, notwithstanding its high combustible properties. This arises from the circumstance of its melting at so low a temperature, and so large a portion of it, therefore, passing off unconsumed and in a state of vapour, from the difficulty with which it enters into connexion with the atmosphere during the process of combustion. The union of resin, however, with the carbon of the turf has the effect, by virtue of the extraordinary attraction of the latter for oxygen, of furnishing the necessary supply to the resin, and thus giving full combustible and calorific effect to that powerful bitumen. This may be said to take place, more or less, in the combustion of all coal, and forms the main distinction between bitumen and coal.

In the manufacture of the resin fuel, the turf coke, in the state of powder, is added to the resin, or other bitumen, in a melted state, upon which an immediate and intimate union takes place. The proportion in which these ingredients, when pure, should be mixed, is that of equal quantities, by weight.

The price at which this fuel may be manufactured will necessarily vary according to the locality of the manufacture, and the value, quantity, and quality of the ingredients used; keeping these in view, the cost of the manufactured article may average from twenty to forty shillings per ton.

Having tested the value of this fuel in repeated voyages across the Channel, and during five voyages across the Atlantic in the "Royal William" and "Liverpool" steam ships, the result is, that the most economic mode of using it appears to be in the proportion of 2 cwt. to 20 cwt. of coal. If used judiciously and in this proportion, 20 cwt. of coal, and 2 cwt. of the resin fuel, will do the work of 26 cwt. of coal. The proportion of the resin fuel may be increased with good effect, should the pit coal be inferior in quality or of a slow burning nature.

But this saving of dead weight carried, amounting to above 15 per cent. of the entire supply for the voyage to New York, is perhaps the least important of the advantages arising from the use of this fuel. In case of a protracted voyage, some of this fuel, in connexion with cinders and a small portion of coal, will be adequate to keep the engines in operation. Again, by the suddenness of its action and the great volume of flame given out, it enables the

engineer to avoid a deficiency of steam at one period, by which the engines are retarded; and an excess at another, by which a waste of fuel takes place; this irregularity arising from the difficulty of keeping so many furnaces at a uniform intensity.

Again, where it is desirable to suspend the operation of any furnace, or even all the furnaces under any one boiler, to clear out or repair, a small increase of this fuel under the remaining furnaces, has been found adequate to the maintaining a due supply of steam.

In conclusion, I would observe that the result of ten years' experience, during which I have given much attention to the subject, is, that the turf bogs, particularly those of Ireland, which have hitherto been regarded as an incumbrance, possess many valuable properties which qualify them for several important uses in the arts. The application of an improved fuel to the manufactures of this country, and to the purposes of the steam engine, being a subject of the deepest interest, and as the accumulation of facts is the first step towards the acquisition of sound views and improved practice, I have presumed to submit the foregoing to the notice of the Institution.

This subject being far from exhausted, with the permission of the Institution, I propose, on a future occasion, submitting to them some further observations, with the details of such operations as may then have been proceeded with.

CHARLES WYE WILLIAMS.

LIVERPOOL, February 12, 1839.